

**Interviews  
with  
Prof. John Bardeen**



**Interviewed by Lillian Hoddeson  
Collected & Compiled : Symmetry Seeker**



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# Interview Session - 1

## Hoddeson:

This is Lillian Hoddeson. Today's day is May 12, 1977, and I'm interviewing Professor John Bardeen in his office in Urbana, Illinois. John, when we spoke together last week, I mentioned two of my own research interests; the development of basic research, particularly solid-state research in industry, and the development of solid-state physics in America during the 1930's, 40's, and 50's. Your account of your participation in these events will provide excellent source material for my historical study of these developments. However, for the benefit of future historians of science who will read the transcripts of our discussion, I am not going to limit our interview to the particular developments in your life that interest me. I would like to begin by recording some facts that pertain to your early background. I see that you were born in 1908 in Madison, Wisconsin, the son of Charles Bardeen, who was a Professor and later Dean at the University of Wisconsin Medical School from 1907 to 1935. I

suppose, therefore, that intellectual activities were stressed in your home and that you were fairly well off financially.

**Bardeen:**

We were reasonably well off financially. My father's income was all we had to live on. We had five children altogether so I wouldn't say that we were really affluent. We were comfortable.

**Hoddeson:**

Five children?

**Bardeen:**

My father married twice. My mother had four children. She died of cancer when I was about twelve years old and then a year or so later my father remarried. One daughter, Ann, resulted from that marriage. She's the only one who took up father's profession of medicine. She's an M.D., a specialist in anesthesiology, practicing in Milwaukee, Wisconsin. Of the three boys, none of us took up medicine. My younger sister Helen, who was the third child to be born, wanted to go into medicine but my father discouraged her because he thought women would

have an expensive medical education and then get married and have a family and not really use it. I was interested to see that in the first class, which graduated from Wisconsin four-year medical college, in 1827, out of 25 students, six were women. He couldn't have discouraged them too much.

**Hoddeson:**

What did your younger sister go into?

**Bardeen:**

She went into nursing. She entered Yale Nursing School and married a doctor who graduated from Yale Medical School. He came from Canada, British Columbia. Donald Beech was his name. Not long after they moved out there, she came down with tuberculosis. They didn't have the drugs then that they do now. She died shortly after her first baby was born. We went out to see them on our honeymoon trip in 1938. We took an automobile trip out to the West. We stopped in Vancouver and saw them there. She was not well at that time. Not long after that she died which was very unfortunate.

**Hoddeson:**

Indeed. Tell me about your brothers.

**Bardeen:**

My older brother William went into business and during most of his life worked for the Armour meat packing company. He was originally working in the business office, accounting and so on. Then he went into sales. He worked in sales in several cities in Wisconsin and ended up in Rockwood, Illinois. He's retiring now. His last job was head of the branch office of the Armour Company in Rockford, which has since been closed.

**Hoddeson:**

And your younger brother?

**Bardeen:**

Tom married early. In those days it was thought that you should support your wife if you got married. He was very bright, interested in the same sorts of thing that I was. Electrical engineering, mathematics, physics. I was working at the Gulf Oil Company at the time he got his master's degree. He was working



for his doctor's degree, but after he got married he decided he better get a job. So I helped him get a job at the Gulf Oil Company and he spent his career working as a geophysicist at Gulf Oil. He has had a very successful career there, from which he just recently retired. He was responsible for a great deal of their seismic instrumentation over the years.

**Hoddeson:**

So he's been in Pittsburgh all these years?

**Bardeen:**

Yes, for the most part. He now lives in Wyoming.

**Hoddeson:**

And your mother?

**Bardeen:**

My mother's name was Althea Harmer before she was married. She was an art teacher at the Drew Institute in Chicago, which later became part of the University of Chicago. Her specialty was Oriental Art, particularly Japanese Art, which was just coming to the notice of the West. Japan opened up to the West about 1860 or 70. Around the turn of the

century, Japanese Art was to be a big thing, and she was interested in that.

**Hoddeson:**

So I gather your scientific interest probably came from your father's side?

**Bardeen:**

Yes, although my mother pushed intellectual development too.

**Hoddeson:**

I read in Patrick Young's article <sup>[1]</sup> that your mother enrolled you in an experimental school because, he says, she was afraid the public schools were not good enough.

**Bardeen:**

I started going to public school, spending three years, to the grade school in my neighborhood. Coming from the East where many people send their children to private schools, she had some distrust about public schools. Would I get enough intellectual stimulation there? The University High School at Wisconsin was somewhat similar to the

University High School here at Illinois, at least as it was run some years ago. They had a combined seventh and eighth grade and then four years of high school. She put my older brother and me in that University High School. My brother had just graduated from fifth grade and skipped one grade. I had just graduated from third grade and so had to skip three grades.

**Hoddeson:**

This must have been extremely difficult for you all the way through because you were always three years younger than everybody else.

**Bardeen:**

Yes, I was a lot younger than other students which I think was a handicap socially. I did well in mathematics and science but only average in language.

**Hoddeson:**

Did that tendency continue right through college?

**Bardeen:**

I stayed at home. My friends before I went to University who were mostly children of my own age in the neighborhood. So it wasn't so much of a handicap at that time. My brother had friends who were a little bit older so of course I knew them too. Most of my friends were a little bit older than I was, but not as much as three years older.

**Hoddeson:**

How much older is your older brother than you are?

**Bardeen:**

Two years older. We started at the same time at the University High School.

**Hoddeson:**

Then you moved on to Madison Central Public School which had additional math courses.

**Bardeen:**

Yes, that was one reason. My brother decided to switch to the Madison Central High School. I would have graduated a year earlier if I had stayed at the

University High School but I was a little leery about graduating so young. I could get extra courses by going to the Madison Central High School, an extra year of mathematics which I couldn't get at the University High School and also take up other courses and delay graduation by a year. I graduated when I was fifteen.

**Hoddeson:**

Well, that's not all that young.

**Bardeen:**

And then while I went to University, I was still living at home, so the adjustment happened while I still had friends who were about my own age. But at the University, I really had to make a transition to interact with contemporaries as far as the University went. I joined a fraternity.

**Hoddeson:**

Was fraternity life influenced much by the fact that you were younger?

**Bardeen:**

Well I didn't live at the fraternity. I lived at home and went out there for occasional meals. Sometimes I stayed there overnight. I knew the fraternity brothers well, but I didn't actually live at the fraternity, so I didn't take full part in the fraternity life.

**Hoddeson:**

I gather you were well off into mathematics by this time? When was it clear to you that mathematics was definitely an important direction in your life?

**Bardeen:**

Well, more or less the first time I went to University High School. I had a very good teacher in seventh and eighth grade. The teacher was from the department of education at the University, N.W. L. Hart, who is a co-author of the Wells and Hart series of mathematics texts, which were very popular. I think the series was started by the Wells, and then Hart took over. He edited them and then revised them to keep them up to date. He was about 33 then and he kept his series alive for about 50 years;

continually revising them. He was an exceptionally good teacher. He was author of the texts and was much above the average high school math teacher.

**Hoddeson:**

What about other areas of the physical sciences?

**Bardeen:**

I had good teachers in science too. I first took a course in general science from a person whose name was Davis.

**Hoddeson:**

This was at the University High School?

**Bardeen:**

Yes, and he was also a very good teacher. I took general science there. In high school I had biology, chemistry, and physics. However, I took physics at the Madison Central High School. But I think I had general science and geology at the University High School. Oh no, I took biology at the Central High, it

must have been chemistry I took at University High School.

**Hoddeson:**

Was it very clear by the time that you were at the Madison High School that you would go into a scientific career?

**Bardeen:**

I was certainly headed toward mathematics and science.

**Hoddeson:**

Did you do any work with radio or other such things as a high school student?

**Bardeen:**

I did some work with cat's whisker radio detector, that most boys were working with at that time. Some boys got as far as putting in vacuum tubes in their amplifiers but I never got that far. My main project in high school days was doing chemistry in my basement laboratory. I got interested in that by reading a book on Creative Chemistry written by Slosson. During the first World War we were shut



off from importing dyes from Germany; the organic chemists in this country had learned how to produce the dyes. And that was described in this book. So I got interested in how dyes were made and I made some. I dyes materials, did some experiments on ejecting dyes in eggs, seeing now you get colored chickens (laughter) and things of that sort. Nothing too elaborate.

**Hoddeson:**

Was the book you mentioned written for younger people or for general readers?

**Bardeen:**

It was a general popular text. No it wasn't a text, it was just a book.

**Hoddeson:**

Did you buy that book yourself, or did your mother pick it up for you?

**Bardeen:**

I probably bought it for myself. I don't remember. Maybe in taking chemistry I learned about it. I do remember being stimulated by the book.

**Hoddeson:**

Your mother passed away while you were in the last year at Madison Central High School.

**Bardeen:**

I was twelve years old so I was then still going to the University High School.

**Hoddeson:**

Did that cause you to take time off from your high school work? It must have been a tremendous shock. You were so young.

**Bardeen:**

It must have been very difficult for her, leaving four children behind. She had some friends from her University of Chicago days and she went down to live with them for a while and I went down to visit her there. That must have been the summer because we weren't losing any school from it. Then she came back and spent her last days at the University Hospital. It was on the way between my high school and home so that I would stop and see her on the way home from school. I remember stopping in to

see her on the day before she died. I thought she looked well that day and cheerful and I was shocked to hear the next day that she had passed away. I didn't realize how seriously ill she was.

**Hoddeson:**

It's good that you were able to see her just before she left, that she was nearby. After a period, your father remarried; he married Ruth Hames. That must have been a difficult adjustment.

**Bardeen:**

It must have been difficult for her and also for the children. I spent the summer after she died with some relative in Michigan. My younger brother and sister went to Syracuse and stayed with my aunt who was living with my grandfather in Syracuse. So we got along through the summer that way. We had hired help at home, a housekeeper. My father married Ruth Hames about a year later and she looked after us very well, but I'm sure it must have been a difficult job for her. She not only had four children to look after, but had a daughter herself a year or two after the marriage. It was rather a difficult job for her. She's in her mid-eighty's now.

She lives in Milwaukee. She got married again just after the War to a man she met when she was a student at the University many years earlier. He had never gotten married; his name is Ken McCauley, who was an editorial writer for the Milwaukee Journal. He's about the same age as my stepmother, mid-eighties. She's getting rather frail and declining but he never seems to change. He's just as alert and healthy as he's ever been.

**Hoddeson:**

You graduated high school at age fifteen in 1923 and then made the decision to go to the University of Wisconsin. Were there other choices?

**Bardeen:**

Well I think that was pretty well set, partly for financial reasons and partly because of my age. I thought best to go to the local University. I took electrical engineering because I had heard that that used a lot of mathematics.

**Hoddeson:**

Eventually you worked with Van Vleck, toward your master's degree. Is that correct?

**Bardeen:**

Well, I didn't work with him; I took a course under him. My master's degree was in electrical engineering. He was in physics.

**Hoddeson:**

What courses did you take with Van Vleck?

**Bardeen:**

I took a course in Quantum Mechanics in 1928 when I was a graduate student. I started college in 1923 and took a lot of extra courses in mathematics. As an undergraduate, I did a great deal of independent study in mathematics, much under the guidance of Warren Weaver, who was later head of the Rockefeller Foundation.

**Hoddeson:**

How did Warren Weaver happen to be your teacher?

**Bardeen:**

He was then a young professor in the Mathematics Department at the University. I started taking a regular freshman course in analytical geometry

given by a teaching assistant at the university. I started taking the regular course of first year math for electrical engineers which started out at that time with analytical geometry. But soon I started studying on my own, learning calculus, because I knew most the things that were being taught in the elementary course. I took advanced courses with Van Vleck's father and one with Warren Weaver. I took many other courses in mathematics and also did a lot of studying on my own.

**Hoddeson:**

Was Van Vleck's father a regular mathematics professor at Wisconsin?

**Bardeen:**

Yes, his father was a regular mathematics professor at Wisconsin?

**Hoddeson:**

And then young Van Vleck came too and taught quantum mechanics?

**Bardeen:**

He came later; he wasn't there at that time. I was an undergraduate. He took his Ph.D. from Harvard and then went to the University of Minnesota where he stayed a couple of years. He came to the University of Wisconsin in 1928 and taught a course in quantum mechanics. I had, earlier when I was an undergraduate, also taken a lot of graduate courses in physics as well as in mathematics.

**Hoddeson:**

What did you take, the basic courses such as optics, thermodynamics, mechanics, and so on?

**Bardeen:**

There were certain required courses that the electrical engineers were required to take. I took those and then also extra courses at the graduate level that were being offered. I remember one course that was given by Debye. He was visiting Wisconsin for a semester and he gave a course in statistical mechanics. It was a very stimulating course.

**Hoddeson:**

What did he cover in that course?

**Bardeen:**

Kinetic theory and statistical mechanics.

**Hoddeson:**

I suppose there was not much quantum theory in this Debye statistical mechanics course.

**Bardeen:**

No, there wasn't much quantum theory in that course.

**Hoddeson:**

Did he cover any of his own research in that course?

**Bardeen:**

No, I don't think so. It was mainly just a basic course. Kinetic theory and statistical mechanics. I think I still have the notes from that course.



**Hoddeson:**

Please don't throw them away. Historians will find them very interesting.

**Bardeen:**

He was of course an excellent teacher. He had everything very well organized.

**Hoddeson:**

This was before you took quantum mechanics?

**Bardeen:**

Yes, this was before I took quantum mechanics. That was one of the more stimulating courses I had. I took most of what was offered, either in my later undergraduate or graduate years. Another extra course I took was a year of German, which I thought I might need later when I went to graduate school. It threw my electrical engineering program out of kilter. I had to put off some of the required courses because I was taking German. There were many required courses in the junior and senior year at that time and few electives. I don't know if it's still true. You were supposed to spend a summer working in

industry. Certain industries used to take summer students and the University had connections to them. I worked with the Western Electric Company in Chicago in the Inspection Development Department. That was the summer of 1926. Then I took a semester off also to work at Western Electric. I enjoyed the work there and being young I was in no hurry to graduate. Also other graduate courses I had taken in mathematics and physics caused my electrical engineering program to be thrown out of kilter, so I decided to stay out for a semester and continue to work in Chicago, which I did. So I was there for about 8 months altogether.

**Hoddeson:**

What did you do there?

**Bardeen:**

I worked developing methods for inspecting. When there were special things that were turned out in very small quantities we would be responsible for inspection tests.

**Hoddeson:**

Did you have any interactions with Bell Laboratories in that period?

**Bardeen:**

Not at that time. The job required more engineering than research, although it was not routine. It consisted of developing methods for inspection and inspecting non-routine items. There were always new problems coming up so it was interesting work.

**Hoddeson:**

These were experimental problems?

**Bardeen:**

Yes, they were largely experimental problems.

**Hoddeson:**

So you got your hands into experimental work there.

**Bardeen:**

Well, it was not ordinary experimental work. It was more designing tests to see whether production meets specification. After spending a semester at

Western Electric, I returned to the University in the spring of 1927. My class graduated in 1927, but I didn't because I didn't have all the required courses completed. I spent an extra year and graduated in 1928. I took some graduate courses during the year then I stayed on as a graduate student in electrical engineering after that. I was there for two years beyond the B.S. degree.

**Hoddeson:**

You also studied quantum mechanics with Dirac. Was this as an undergraduate?

**Bardeen:**

That was in the summer of 1928, I believe. It was just before Van Vleck came from Minnesota. Dirac was in Madison for six weeks in the summer. He gave lectures in quantum mechanics which I took. His course followed pretty much what was in his book, published a little later.

**Hoddeson:**

How did this impress you?

**Bardeen:**

It was very stimulating. Very interesting. I enjoyed it. I thought of transferring to physics and doing research. I was never quite sure that that's what I really wanted to do. My graduate work was in electrical engineering. One problem was on calculating radiation from antennas. At that time, many people from the University and elsewhere were becoming interested in geophysics which had just opened up at that time.

**Hoddeson:**

I have the name Leo J. Peters written down here.

**Bardeen:**

Peters was one of those interested in geophysics. I did my master's thesis under him on the problem of electrical methods in prospecting in geophysics. The other problem, suggested by the head of the department, was on diffraction of electromagnetic waves. It related to the antenna design. Peters left in 1929 to go to work for the Gulf Oil Company. I stayed on another year at the University, mainly

taking extra work, since I'd finished my master's thesis.

**Hoddeson:**

On the diffraction of electromagnetic waves?

**Bardeen:**

It was the other way around. When Peters was there, I worked on my master's thesis. I got my master's degree in '29 and I must have worked on the other problem in '29-'30.

**Hoddeson:**

The prospecting problem?

**Bardeen:**

No, on electromagnetic waves. I thought of going to Germany which was then the center of physics research.

**Hoddeson:**

This was in 1929?

**Bardeen:**

In 1930. The academic year, 1929-1930.

**Hoddeson:**

Did you have a specific invitation to go to Germany at that time?

**Bardeen:**

No. I didn't apply. I did apply for a Fellowship in Cambridge which I didn't get. That was the only Fellowship I applied for.

**Hoddeson:**

Where were you thinking of going in Germany?

**Bardeen:**

On one of the main centers like Göttingen or Munich.

**Hoddeson:**

Had you become informed in detail about the work that was going on at these centers?

**Bardeen:**

I was pretty well informed through Van Vleck. Sommerfeld was at Munich.

**Hoddeson:**

Were you already at this time aware of Sommerfeld's 1928 theory of metals?

**Bardeen:**

It had just appeared. Sommerfeld had talked about electrons in metals at Wisconsin.

**Hoddeson:**

Here in this country?

**Bardeen:**

Yes, he was visiting during part of the year 1929-30. I think it was in the spring of 1930 that he visited Wisconsin. He talked about the electron theory of metals. I heard the lectures but I wasn't stimulated at that time to go into that field. But I was thinking about going to Germany to work on theoretical physics. I think my main strength was mathematics. I leaned towards mathematics and theoretical



physics. Then Peters who had gone to Gulf Oil Company wanted me to join him there. And a recruiter from Bell Labs, Thornton Fry, who visited in the spring of 1930, was encouraging about a job at Bell Labs. So I decided to take a job.

**Hoddeson:**

There were these two possibilities for you in 1930, Gulf and Bell Laboratories. And you chose Gulf because you knew Peters?

**Bardeen:**

Oh, I chose because I didn't really have any choice. I may have chosen it anyway but between April of 1930 and the summer in June when I went out to visit, Bell cut out all new employment, except for those who had a fixed contract. They were not making any offers; in fact they didn't hire any new science employees, scientists or engineers between the spring of 1930 and 1936. But these were the days when geophysics was just opening up. It was an interesting field, the methods were being developed. I may have chosen that job anyway.

**Hoddeson:**

What was the job that Bell at that time offered to you?

**Bardeen:**

I think it was to work on antennas. The interest was in wave propagation and antennas. If I'd gone there I might have ended up working in that field. They had some very good people in the field.

**Hoddeson:**

I'm surprised the job at Gulf was still open despite the depression.

**Bardeen:**

It was one of the few places that were still hiring. Oil companies were still reasonably prosperous even in depression days. People had to buy gas to run their cars and geophysics was just opening up, so it was an expanding field. They did some hiring even after I arrived.

**Hoddeson:**

Did you experience any restrictions due to the depression at Gulf?

**Bardeen:**

No, they were expanding in terms of the numbers of employees and activities. I was working mainly on electromagnetic prospecting, and interpretation of magnetic surveys. But I also did some work on electrical methods and gravitational methods and seismic methods. Those were very interesting days. While I was there, I also participated in a seminar at Pitt (Univ. of Pittsburgh) Arthur Ruark, among others was there. Different people would report on current activities in theoretical physics. So I kept up my interest in that field by attending these seminars. They also had speakers from out of town.

**Hoddeson:**

Do you recall any of the speakers at these seminars?

**Bardeen:**

No. In about 1933 I decided that if I were going to stay in geophysics I would have to learn more

geology and go in that general direction. I'd never studied geology. After some thought I decided to go into mathematics and theoretical physics, which was my real interest. I picked Princeton because the Institute for Advanced Study was just starting there at that time. There were a lot of famous mathematicians there and looked like the best place in the country to go. I had enough savings so I didn't need a fellowship.

**Hoddeson:**

Would you call the work you were doing at Gulf entirely applied work, or was there a basic component to it?

**Bardeen:**

I was trying to develop new methods of interpretation. They were applied problems in that you were given the survey of fluctuations in the earth's magnetic field as they vary over the earth and then you would try to infer from these what was causing the fluctuations and develop indirect methods of analysis. One may assume that the rocks that give rise to the magnetization are uniformly magnetized and then one can calculate what

structure would give rise to the field observed on the surface. We worked out a direct way for going from the magnetic field observed to the structure below on the assumption of uniform magnetization. But of course the assumption of uniform magnetization is by no means valid. That is a big problem in the field. I also worked out what I think is more valuable, a way of estimating the depth to the basement of rocks which cause the variations in magnetic field. Sedimentary rocks generally don't have much magnetization. The field comes mostly from the basement rocks, and one can determine roughly the depth down to these rocks. If one doesn't know much about the area from a magnetic survey, one can get a rough idea of the geology and thickness of the sedimentary rocks. We developed methods for doing this. I was in charge of a small group on magnetic interpretation. We'd get the results in from the field and try and interpret them.

**Hoddeson:**

Were you reading journals at that time or were you relying mainly on books?

**Bardeen:**

This was a new field so there was not much in the literature. There was an article which Peters wrote and which was published I think in the late '40s describing the work that we did in the early '30s, 1930 to 1933. Peters was in charge of the overall geophysical research, not only magnetic but also gravitational and seismic. I was in charge of the magnetic activities, though I did work out problems in the other areas too. I wrote a joint paper in that period on electrical methods in prospecting.

**Hoddeson:**

This is a good place for us to break for lunch. My next questions concern your transition from Gulf to Princeton and that will undoubtedly take us some time to discuss.

**Bardeen:**

Yes.

## Interview Session - 2

**Hoddeson:**

We are resuming on May 16 beginning in the year 193 when you made your transition from Gulf to Princeton. You were last time going to tell me what lead to that decision.

**Bardeen:**

As I was saying before, I found the work at the Gulf Laboratories very interesting. It was the early days in geophysics when lots of new ideas were under development and lots of new physics was involved. I realized that if I wanted to do geophysics in the long run I would have to learn more geology. As I said last time, I had been attending a seminar at the University of Pittsburgh trying to keep up to date in the developments in physics in that period.

**Hoddeson:**

How large was that seminar?

**Bardeen:**

Oh, it was just the usual seminar, it wasn't very large, at most 8 or 10 people, faculty members from Pit and Carnegie Tech. And a few graduate students. I think Arthur Ruark was the guiding spirit. He was there at that time.

**Hoddeson:**

Was your attendance at this seminar supported by Gulf or did you attend on your own time?

**Bardeen:**

It was on my own time, outside of regular working hours. The decision to move on to Princeton was a difficult one, because it was 1933 when jobs were hard to get. I had a good job at Gulf. I didn't know if I would be able to get a good job again if I quit this one to go back to school. But I decided to and I left to proceed with my real interests. And as I said, I was going on my own money so I could pick the University where I wanted to go and I picked Princeton because there was an outstanding mathematics department there as well as the Institute for Advanced Study, that had just gotten started



there a couple of years before. They had some of the leading mathematicians in the world and some leaders in theoretical physics.

**Hoddeson:**

Did you receive any outside support during any part of your Princeton period?

**Bardeen:**

I got a Fellowship in my second year, but I did the first year on my own. I was there two years. I'd had a master's degree from the University of Wisconsin, and so I did have a reasonably good background in mathematics and physics. I picked mathematics; I joined the mathematics department because I thought my mathematics was stronger than physics. I still wasn't sure I wanted to go into physics. And Fine Hall at Princeton, where the mathematics department was housed at that time, was right near the physics department. And at that time, the Institute for Advanced study was located there. They didn't have their own building at that time.

**Hoddeson:**

And physics was then in Palmer?

**Bardeen:**

Yes, that was right next to mathematics and there was a joint break for tea time every afternoon.

They'd have tea about 4:30 and the people from both mathematics and physics would come. There was close cooperation between the two departments and also with the Institute for Advanced Study. The people at the Institute gave advanced seminars which were over my head but I went to some of them anyway. In the first year Von Neumann was giving one which I attended. I also attended others. The people in the department I might have been interested in working with were Edward Condon, who was then involved in writing his book on atomic structure which has since become a classic in the field, Condon, Shortley, and Robertson, in general relativity, and Eugene Wigner who was then spending half his time at Berlin. I first talked with Condon about a research problem -- this was the first year I was there. He was involved in his book and had some problems of the kind that would fill gaps

in the book. They didn't sound too interesting to me. I also talked with Eugene Wigner, and the sorts of things he was doing sounded more interesting. So I started doing research with him.

**Hoddeson:**

How close was your student-professor relationship with Wigner?

**Bardeen:**

It was very informal. There were not very many graduate students there then and there was no difficulty in talking with him. I probably saw him once or twice a week when he was in residence there.

**Hoddeson:**

Did Wigner specifically suggest that you work on the work functions of metals?

**Bardeen:**

Well, I had been working in a different area.

**Hoddeson:**

Which area?

**Bardeen:**

In quantum electrodynamics. It didn't look like that work would get too far. I was taking a course in solid state theory and there was a lot of interest at that time in this area at Princeton. Fred Seitz was there; he had been there a year or two before I arrived; he had already essentially completed the research on his thesis and he was continuing on with further problems. That was the time Wigner and Seitz developed their method for calculating wave functions for electrons in crystals in a realistic way-- that is making calculations for actual metals rather than the normal type of theory which was just calculating with wave functions which weren't specific to any particular metal. They were able to calculate the binding energy for simple models like sodium and lithium and this was a breakthrough in the theory. It looked like it would open up a new area.

**Hoddeson:**

Was it obvious to many people at that time that this work was indeed a major breakthrough?

**Bardeen:**

Well, it was hard to tell how far it would go, but looked like it would be opening up a new area to be able to calculate for actual metals rather than for just idealized metals, and also to calculate the nature of the metallic bond. This happened in the first year that I was there.

**Hoddeson:**

Did you get very much involved in the actual details of the Wigner-Seitz work at the time it happened?

**Bardeen:**

Oh, I wasn't involved in that directly. Only afterwards, when I really started working on my thesis on the work function of metals, during the second year I was there.

**Hoddeson:**

But the Wigner-Seitz work did help to direct your interest?

**Bardeen:**

Yes, that was the motivating force.

**Hoddeson:**

Were you friendly with Seitz in that period?

**Bardeen:**

I knew him very well. We both lived at the graduate college at Princeton. We were very good friends. Also there was Joe Hirschfelder who was working on chemical problems. Wigner later had other students but only during a short time was he interested in solid state problems, 3 or 4 years. He had a number of students; Conyer Herring was another of them. We overlapped one year; he came the second year I was there. I made good progress on the thesis and thought I would stay on another year at Princeton, but then I was offered a Junior Fellowship at Harvard. This was due to the influence of Van Vleck, who had since moved to Harvard from Wisconsin and he knew me from my Wisconsin days.

**Hoddeson:**

Before we move on to Harvard, I have a few more questions on the Princeton period. You mentioned the course which Wigner gave on atomic physics

and solid state theory. I wonder if you could tell me a little more about that course. For example was there a text. Did you perhaps use the Bethe-Sommerfeld Handbuch article?

**Bardeen:**

Well, it was more his own notes. It gave the general background, the Sommerfeld theory, the Bloch theory, I think even something on alkali halides. So it wasn't confined only to metals.

**Hoddeson:**

Did you read original papers in that course?

**Bardeen:**

It was more a normal graduate course. We read review articles rather than original papers. There was no text at that time.

**Hoddeson:**

Was this one of the first courses on the new quantum theory of solids in the United States?

**Bardeen:**

It was certainly one of early ones. Van Vleck undoubtedly gave courses, but his interests were more in crystal fields, magnetic properties, things of that sort, that were somewhat different from Wigner's interests. Courses that he gave would be directed more along those lines; energy levels of atoms and crystals, crystal fields, and things of that sort. Slater might have given courses too. Certainly he did later, I'm not sure whether he did in that very early period.

**Hoddeson:**

I find this particularly interesting. It appears as one of the nuclei from which the field of solid state physics in this country developed.

**Bardeen:**

There were three areas in which it got started in the United States. There were many groups in Europe involved in it but in the United States it was Van Vleck, Slater, and Wigner. Slater was more interested in magnetic properties, dielectric properties and things of that sort. Van Vleck's book



of 1932 was one of the books we studied, but in general it was reading review articles. We talked with people first hand and went to lectures.

**Hoddeson:**

About how large was the course?

**Bardeen:**

Oh, not many. I would say there might have been a dozen students there, if that many. A dozen students sitting in, don't think they were all taking it for credit.

**Hoddeson:**

And that group included you, Seitz, and Herring?

**Bardeen:**

I think I took it the first year. Herring did not arrive until my second year. I didn't consider myself a solid state theorist. There weren't any solid state theorists at that time. You were in theoretical physics. Theoretical physics didn't begin to split up into specialties until later.

**Hoddeson:**

Was the word solid state used at all at that time?

**Bardeen:**

No, not until later. I think use of the words solid state really got started after the War. I don't think it was generally used before the War.

**Hoddeson:**

Was there a feeling that this was the beginning of a tremendous new field that would eventually mushroom?

**Bardeen:**

I didn't think it would to the extent that it actually did. There were a lot of good problems to be solved in the field. I didn't feel at all committed to spending all of my time in that field then.

**Hoddeson:**

What were some of the other possibilities at that time for you, areas you thought you might work in at that time?

**Bardeen:**

Well, as I said, I was beginning to work at quantum electrodynamics. There was the problem of all those infinities that you run into in electrodynamics. And I tried summing terms in perturbation theory and things of that sort. We didn't make a whole lot of progress. The key ideas came later.

**Hoddeson:**

It was early, granted it was early, but did you happen to study any of the work of Mott, Schottky, Wagner and others in this period pertaining to rectification?

**Bardeen:**

I studied Mott's book which came out later in 1936. I studied it while I was at Harvard.

**Hoddeson:**

You mean Mott and Jones?

**Bardeen:**

Yes.

**Hoddeson:**

But then, semiconductor work generally came later for you, is that correct?

**Bardeen:**

Yes, I didn't do anything on semiconductors until after the War.

**Hoddeson:**

One other question on the Princeton period. I learned from Jim Fisk about lively exchanges between the groups working then in Cambridge, at Harvard and MIT, and the Princeton group. He recalls visits between members of the Princeton physics department who would come up to Cambridge and members of the Cambridge community who would come down and visit the researchers at Princeton. Do you recall similar exchanges?

**Bardeen:**

There were a lot of exchanges, but they came later. The number of people involved was too small then. In the first year I was at Princeton, it was just Fred Seitz and myself. The next year, there was Conyers

Herring but he was just a first year graduate student and wasn't yet doing much in the way of research. And probably similarly at MIT and Harvard, the groups were still too small then in that very early period to provide much in the way of interaction. After I went to Harvard of course, I interacted closely with the people at Harvard, with Van Vleck, and in experimental physics with Bridgman. I also saw the people at MIT frequently.

**Hoddeson:**

While you were at Princeton, did you participate in any informal study groups with other students, who were there when you were there?

**Bardeen:**

No, not during that period. There were a lot of seminars given by people at the Institute.

**Hoddeson:**

Whom at the Institute at that time did you interact with?

**Bardeen:**

Since I was most interested in the theoretical end, remember most Van Neumann and Veblen. There were others who were more mathematical with whom I didn't interact with very much.

**Hoddeson:**

Would you as a graduate student be able to talk with these people at tea?

**Bardeen:**

I'd talk with them. I'd generally show up for the afternoon tea and many of them did. I'd see them on other occasions. I don't think I talked to Van Neumann or Veblen too much. What they were talking about was somewhat beyond me at the time I was taking the seminars. I was just getting a little feeling for some of the ideas that they were talking about. I took a course in General Relativity from Robertson, General Relativity and Cosmology, a very good course. At the time I was at Harvard I was asked if I wanted to give a course in General Relativity and I gave a course using Robertson's notes and Eddington's book.

**Hoddeson:**

I'd like to ask one more questions about the Princeton Period. You've mentioned in one of your writings that it was in that period that you first met Walter Brattain.

**Bardeen:**

His brother was a graduate student who arrived the same year I did at Princeton, Walter's younger brother. And I knew him. He was a graduate student in Experimental Physics. I think he was working on problems in the infra-red. I met Walter through his brother in an apartment in New York. I went in to visit him occasionally.

**Hoddeson:**

Walter Brattain was, I believe, already in this period working on semiconductors at Bell. The copper oxide work was just beginning. Did you talk about this with him then?

**Bardeen:**

I didn't really talk that much physics with him. It was more of a social acquaintance. I'm sure I didn't

get involved in thinking about any problems in semiconductors at that time.

**Hoddeson:**

Well, then you went to Harvard.

**Bardeen:**

Then I went to Harvard. One reason for going was I was offered a Proctor Fellowship at Princeton which was their top Fellowship and paid very well, but the Fellowship at Harvard was for three years; guaranteed employment for three years. It paid even better in fact than the fellowship at Princeton, \$1,500 plus living expenses at one of the houses at Harvard. At that time that was very good money. So my future would be assured for the next three years there. It would have been more of a gamble if I'd stayed at Princeton.

**Hoddeson:**

How did you get the position at Harvard? Do you know who recommended you?

**Bardeen:**

I think probably Van Vleck did.



**Hoddeson:**

Was that a position you applied for?

**Bardeen:**

No. They interviewed you. They gave a personal interview. When I got the appointment, I was really overawed by these dignified professors, people who didn't know much about the field I was working in. To get these Fellowships one needs strong recommendations from people who know you and Van Vleck was the only one I knew at Harvard. So it must have been him. It must have been mainly due to his influence.

**Hoddeson:**

Do you remember who some of the persons who interviewed you were? Were they in physics?

**Bardeen:**

No, I don't think there was anybody in physics. The Senior Fellows who interviewed me were a very distinguished group. They included Whitehead and Samuel Eliot Morrison. H.J. Henderson was the head of the group at that time. His interest was in

medicine. I don't think there was anybody in physics.

**Hoddeson:**

How large was the group of Junior Fellows? How many, for example, were there in a given year?

**Bardeen:**

It had just started two years before.

**Hoddeson:**

In '33?

**Bardeen:**

Yes, I think so. There were seven or eight appointed the first year and seven or eight, I think, appointed the second year. I think they hoped to build it up to around 20. It was to average about 20 and I was in the third class with about seven appointed each year.

**Hoddeson:**

How many of these seven were in physics?

**Bardeen:**

There were three in physics; Jim Fisk, who was later President of the Bell Laboratories, and Evan Getting, who is now head of the Aerospace Laboratories in California. He got involved in military problems during the war and then stayed in that general area. He was more of an experimentalist. Jim Fisk and I were both theorists. Fisk and Getting both came from MIT. I think Getting came the second year I was there. Jim Fisk, think, came the same year that I did.

**Hoddeson:**

What were your responsibilities as a Junior Fellow other than carrying out research?

**Bardeen:**

Well, the idea was to give promising young students free time to do whatever they wanted in their research, so there were no obligations. We met every day for lunch with other Junior Fellows who were from all fields. Once a week there was a dinner at which the Senior Fellows (Professors) were present. It was a lively group of young people. And very

stimulating both for the Junior Fellows and for the Senior Fellows.

**Hoddeson:**

Did you present seminars to one another in this group, or did that not make sense since you were all from different fields?

**Bardeen:**

When we were together, we talked about work in different fields, about ideas people had in different fields. It was necessarily on a very general level. I think I probably found Whitehead the most stimulating in that group of Senior Fellows at that time. He had a very broad knowledge of practically everything and he had something interesting to say on almost any subject. They weren't pedestrian ideas at all. I think he was very stimulating to all of the Junior Fellows.

**Hoddeson:**

Would he come regularly to the dinners?

**Bardeen:**

Yes, he would come regularly to the dinners.

## **Hoddeson:**

Now your detailed work was with Bridgman and with Van Vleck.

## **Bardeen:**

That came later. One of the most important things was that this was the first time that I was in a physics department. Earlier, I'd been in mathematics and wasn't quite sure whether I'd go on in applied mathematics or theoretical physics. There I was attached to a physics department. I knew many of the mathematicians. Carrett Pirkhoff was one of the Junior Fellows. His father was a Senior Fellow. We had lots of interactions with lots of the people in mathematics as well. We were in physics but we weren't tied nearly as closely with mathematics as we were at Princeton. We were physically right next to each other and the cooperation was very good between mathematics and physics. In most places it's separated. Mathematicians go their own way and interactions aren't nearly as close. At that time at Harvard, they were reasonably good; interactions between mathematics and physics.

**Hoddeson:**

I would like to know some details about the physics seminars at Harvard that you attended in this period and about any courses either at Harvard or MIT.

**Bardeen:**

I think it was mostly seminars and talks.

**Hoddeson:**

Do you recall any specific talks that were particularly influential in your case?

**Bardeen:**

Van Vleck gave a course which I took, but I don't think it was just the courses. It was mainly the free time to read widely in physics. At that time it was possible to read all the important papers which came out. There were a few journals like *Zeitschrift Fur Physik*, and *The Proceedings of the Royal Physical Review*. You could keep up with what was going on in all fields, which of course is no longer possible today.

**Hoddeson:**

Was there a journal club or some other way of discussing these papers amongst your group, or generally with others in the field around you at Harvard?

**Bardeen:**

I think there was just the weekly colloquium. The other was more just informal discussions. Of course the group was much smaller, the department was much smaller, the number of graduate students was much smaller. Now there are all the other graduate students and people on the postdoctoral level.

**Hoddeson:**

You mention that you were able to study widely while at Harvard and that these studies included the book by Mott and Jones.

**Bardeen:**

Mott and Jones came out about 1936 and I studied that.

**Hoddeson:**

Do you remember some of the other books or papers that you read in that period that made a strong impression on you?

**Bardeen:**

Well, of course there was Bethe's article in the Handbuch der Physik. We all studied that.

**Hoddeson:**

Did you study that at Princeton or at Harvard?

**Bardeen:**

Oh probably both. It came out I think while I was still at Princeton. I also studied it at Harvard.

**Hoddeson:**

It appears to have had a major impact on solid state theory.

**Bardeen:**

I thought that I ought to try to apply some of those methods that I learned at Princeton to some of the problems that they had at Harvard. Bridgman was



making a number of measurements on the properties of matter, on high pressure and so on. The only problem I did there was to try to calculate energy of lithium and sodium as a function of volume and also pressure. Of course I had to finish up my thesis which I hadn't finished before I left Princeton.

Wigner was away during the last semester I was there. He was at Berlin during that time so there was no one to check my thesis. One problem was that you're not supposed to work towards a Ph.D. while you are a Junior Fellow. They made an exception in my case; they let me finish my thesis. I think I finished it by about Christmas time.

**Hoddeson:**

Who did finally check your thesis <sup>[2]</sup>?

**Bardeen:**

Oh, Wigner did. He was back at that time then. The problem was to use Hartree-Fock methods to calculate the surface of the metal, to calculate the distribution of electrons at the surface using many-body wave functions. A somewhat similar approach later led to the paper on the theory of conductivity of monovalent metals, which also used functions which

involved many-electron wave functions of the electrons in a crystal. For the surface of a metal, we were dealing with such functions. It is a problem involving all of the electrons in a crystal which we solved by Hartree-Fock methods. In calculating the conductivity of monovalent metals, there is a varying field using the atomic vibrations, the vibrations of the ions which produce a field which is fluctuating in time. I think this is the first application of what people now call time-dependent Hartree-Fock.

**Hoddeson:**

It's interesting to me that your interest in surfaces goes right back to your thesis. To summarize then, you worked both in nuclear and solid-state physics while at Harvard.

**Bardeen:**

I was really applying similar methods to nuclei -- to calculating the level density of heavy nuclei which one can treat by many-body methods.

**Hoddeson:**

How closely did you work with Van Vleck? For example, on the tight binding work?

**Bardeen:**

I think that's the only paper we worked together on. Most of what I did, I did on my own. I would talk to Slater at MIT, and knew Shockley who was a graduate student there. He was interested in surface problems too. So I met both Fisk and Shockley there. Fisk, of course, I knew well. We had lunch together everyday. I knew him better than most because we were both in physics.

**Hoddeson:**

Wasn't Fisk then working in nuclear physics?

**Bardeen:**

He wasn't interested in a variety of problems. I don't think that he was particularly in nuclear physics. He did a problem in nuclear physics for his Ph.D. He was then later interested in problems of concern to biologists, for example, how bats navigate by a sonar type system. I think Fisk was more free ranging than I was. He went from nuclear physics to hearing in bats.

**Hoddeson:**

In this paper on the compressibility of Alkali-Halides in July, 1933<sup>[3]</sup>, I see a trend that seems to be repeated in many of your papers. The work was directly stimulated by experimental results, in this case Bridgman's.

**Bardeen:**

I talked with Bridgman quite frequently about what sort of measurement he was interested in and tried to see whether I could calculate from first principles. It required some semi-empirical theory. He was able to account for the data pretty well.

**Hoddeson:**

I see that you were still in direct communication with Wigner in '38. On page 373, there's a reference to Wigner. Would you still send him your work to look at?

**Bardeen:**

I would go to meetings, stay in New York, and usually go out to Princeton and spend a couple of

days there. The meetings weren't frequent, maybe two or three times a year.

**Hoddeson:**

At the end of this paper you also mention Van Vleck as well as Wigner.

**Bardeen:**

I had more interaction with Van Vleck than anyone else there. He was the first one there who was interested in solid state, and so I'd talk about work with him and did the work pretty much on my own.

**Hoddeson:**

You mentioned Shockley. Did you do any work with him while you were in Cambridge in this period?

**Bardeen:**

No, I didn't work with him. I knew him. We went to joint seminars, I talked about his work with him, but we didn't work together. He was a graduate student there, just finishing up his thesis. One problem he was doing then was trying to calculate wave functions for alkali-halides which you usually think are just made up of sodium and chlorine ions, but

can also be treated from the Bloch point of view. He happened to treat from the Bloch point of view.

**Hoddeson:**

And you discussed this with him?

**Bardeen:**

Yes, I discussed this work with him. And also his work on superstates which he did at that time. He did work which essentially showed that you have bands that cross. From the chemical point of view it corresponds to having dangling bands -- band which are not occupied by two electrons, only one at the surface. They call them dangling bands to represent surface states. And this would occur if in the formation of valence bonds, as they move the atoms together, the bands, say the p-band and the f-band which would cross and form valence bonds, according to the tetrahedral bonds, germanium and silicon. I don't think they were thinking about germanium and silicon at that time. In general, Shockley showed that if you have crossing bands, you can have states at the surface which in chemical language are the extra-orbitals left over. The other valence bonds are filled with electrons.

**Hoddeson:**

You also worked on symmetry effects in nuclear energy levels with Feenberg at Harvard. <sup>[4]</sup> Was that a close collaboration?

**Bardeen:**

Well, we worked closely together. That was kind of an extension of this work on level density. It took it one step further. In this paper, we calculated the level density for a particular value of the orbital angular momentum. We found the density of states corresponding to  $j=0$ ,  $j=1$ ,  $j=2$ , etc. Wigner showed that isotopic spin would be a good quantum number. That is, forces between the protons and neutrons (strong forces) are essentially the same except for the extra charge on the proton. And so the isotopic spin should be a good quantum number. So we tried to extend the level density calculation using group theory methods to states corresponding for a given isotopic spin. I think it was too elaborate a calculation for its time even then..

**Hoddeson:**

What position did Feenberg have at that time?

**Bardeen:**

I think he was a postdoctoral fellow at Princeton. He got his degree in the early '30s and spent a year or two abroad.

**Hoddeson:**

Did you also work on cohesion in this period? That was mentioned somewhat, but I don't see it reflected in any of your papers?

**Bardeen:**

Yes, in the compressibility work. I calculated the energy versus volume of sodium and lithium, which required many hours of work with a hand computer. It could be done very easily now using electronic computers, but the calculations were very lengthy. It turned out that we used the wrong field for lithium. Seitz had published his calculations for lithium using an effective field for the ions which was adjusted to give the correct atomic spectrum for lithium. By mistake, he published the preliminary version of the



field rather than the final field he actually used. I used his published version.

**Hoddeson:**

Oh dear.

**Bardeen:**

Later Conyers Herring found that there was a discrepancy. He was trying to calculate conductivities and in the course of the work, discovered the error.

**Hoddeson:**

Was Seitz aware of what he had done?

**Bardeen:**

No, I don't think he was aware of it. Conyers did a lot of digging to find out. I guess Conyers found some inconsistencies and then tried to check the results in Seitz's paper and didn't get the energy levels which were published using the field which was published. So I spent many hours making calculations with the wrong field. One of the interesting things in connection with the calculation of the compressibilities was making an estimate of

what pressure would be required in cesium to go from a body centered to face centered structure, which was more of a close packed structure. Hard spheres, for example take up a face centered structure rather than a body centered structure. And so it looked like such a change would come from forces between the ions. We made an estimate of pressure at which we would expect this transition to occur. Bridgman looked for this transition and found one pretty close to under the predicted pressure. This was one of the first cases of being able to calculate a structure change before it was found experimentally.

**Hoddeson:**

You mentioned in another interview <sup>[5]</sup> and in some of your writings that in 1936 you first learned about superconductivity having read Shoenberg's book, which years later you gave to Leon Cooper to read. Was this during your time at Harvard?

**Bardeen:**

Yes, that was when I was still at Harvard.

**Hoddeson:**

How did you happen to read the book and would you say this marked the beginning of your since then lifelong interest in superconductivity.

**Bardeen:**

Well, of course, I knew this was one of the outstanding problems, and then Shoenberg's book came out which gave an excellent account of the physics of superconductivity. There was then no adequate explanation. I didn't have any good ideas then. I was just learning what the problems were. It was after I went to Minnesota that I started actually doing some work.

**Hoddeson:**

How much work on superconductivity did you do while at Harvard beyond reading Shoenberg's book?

**Bardeen:**

Well, I read other things too but I think that was the most important work. As I said, I was just trying to learn what the problems were and trying to think if there was anything that might explain it. In general,

during the period at Harvard, I was able to have time to read widely and not feel the pressure to publish. I could read about problems I wasn't planning to work on, problems in all areas of physics. I was trying to keep up with the literature, read all the important papers. I didn't feel committed to working problems in solid state physics except temporarily at that time.

**Hoddeson:**

This is the mid '30s, which was of course a very exciting time for superconductivity.

**Bardeen:**

A turning point occurred in 1935 when the London Brothers came out with their phenomenological theory -- it was the Meissner effect which led to the London Theory. It indicated that you should look for different explanations from one based on absence of scattering of electrons.

**Hoddeson:**

I'd very much like to talk to you about work on superconductivity in the '30s but would prefer to reserve that for later on when we discuss your own work on superconductivity that led to the BCS

theory. I would like to close our discussion of the Harvard period with a question about Jane Maxwell whom you married in 1938. Was this still during the Harvard period?

**Bardeen:**

We were married in June of my last year at Harvard. I left Harvard in July 1938. I met Jane in 1933 at just about the time that I was leaving Pittsburgh. Then I went back in the summer of 1934 to work at Gulf and saw more of her at that time. I'd get back to Pittsburgh occasionally and we would meet. She was at that time teaching biology at Carnegie Tech, at Margaret Morrison College, then the women's part of Carnegie Tech. Later she took a job in a girl's school, a secondary school near Wellesley, also teaching biology, and so I saw a great deal of her during my last year as a Junior Fellow, since she was working close by. We got married just after I finished my term as a Junior Fellow in July of '38. We took a trip out to the West Coast that summer. I got my first teaching job at the University of Minnesota through the influence of Van Vleck. There were a couple of retirements there. Al Nier was at Harvard as a postdoctoral fellow and as a

Minnesota graduate was well-known there. He got one of the jobs and I got the other. So I don't think it was due to Van Vleck's recommendation as far as Al Nier was concerned. They wanted to get him back but I was an unknown. And so it was Van Vleck's recommendation that got me the job there.

**Hoddeson:**

Is this a good place to take a break now?

**Bardeen:**

Yes, I think we should continue at a later time.

**Hoddeson:**

I look forward to the continuation session. Thank you.

## Interview Session – 3

### **Hoddeson:**

Before continuing, I would like to ask you a question I omitted last time when we were discussing your Harvard period, that relates to a comment Foster Nix made when I spoke with him several years ago. He recalled a Cornell summer school in Solid State Physics that he attended during the 1930's but couldn't remember any details about. He, at that time, thought that you might also have attended. If you did, perhaps you could tell me some things you remember about it. It would be interesting to document that, if in fact, it took place because indicates some early interest in the field in the thirties.

### **Bardeen:**

I don't remember any summer school in Solid State Physics. I did visit Cornell to give a colloquium talk, perhaps on more than one occasion, but I don't remember being there for any extended period. The main thing I was interested in talking about at Cornell at that time was trying to apply some ideas of solid state to try to get the level density of nuclei.

Bethe, of course, was the real expert in nuclear structure. I don't remember anything specifically related to solid state.

**Hoddeson:**

Let's return then to where we left off last time. We were at the beginning of your period at Minnesota. You had told me briefly about how you got the job. I would like to know, were there other possibilities for you at that time?

**Bardeen:**

I think there may have been other possibilities, but that was the only really good one, and that turned up through the influence of Vleck, who had been a professor at Minnesota in the twenties, before going to Wisconsin and Harvard. There were two retirements at Minnesota -- Erikson, who had been Department Head, and Zalomi[?] who was a member of the department. Both retired, so they had two openings. One of the post docs at Illinois, I think he had an NRC Fellowship, was Al Nier who worked with me on mass spectra. And, of course, they knew him and wanted to get him back and asked Vleck for other suggestions, and he suggested my name. So I



went out there for an interview. I talked with Jack Tate and others about the position and looked like a good opportunity. So I accepted as an Assistant Professor.

**Hoddeson:**

So you did some teaching then.

**Bardeen:**

Yes.

**Hoddeson:**

What courses did you teach?

**Bardeen:**

Oh, a variety of courses ranging from upper class undergraduate level to graduate level. I think the only elementary course I taught was during the summer; one summer I taught Elementary Physics. In those days, to have the opportunity to teach during the summer and get a little extra money on the side was a real advantage.

**Hoddeson:**

How would you compare the physics program at Minnesota then with that at Harvard and Princeton in the same period?

**Bardeen:**

There wasn't any solid state physics when I was out there except for some experimental work. There was no theoretical work. I didn't go out there thinking of myself as a solid state physicist. I just went out there thinking of myself as a theoretical physicist ready to work on any problems. Some of the work I did there was to try to work out a theory for an isotope separation method which Al Nier was working on.

**Hoddeson:**

Did you get interested in that through Nier?

**Bardeen:**

Yes, through talk with Nier, I got interested in the problem.

**Hoddeson:**

I suppose that on this list, papers number 11 through 15 date from that period.

**Bardeen:**

Eleven is more a holdover from Harvard. Twelve was the one I was just talking about on isotope separation, which was stimulated by experimental work of Nier. This is a review article on electrical conductivity of metals which I wrote for the Journal of Applied Physics.

**Hoddeson:**

I'd like to ask you a question or two about that.

**Bardeen:**

Fifteen is also a result of this work with Nier. Thirteen and fourteen are on metals. I taught graduate level courses in beginning Solid State Physics and also taught upper graduate courses in Electro-magnetism, things like that. I taught a course in Geophysics, taught courses in Atomic Physics -- a variety of courses.

**Hoddeson:**

Did you have any graduate students there?

**Bardeen:**

None that reached the state of getting a Ph.D.

**Hoddeson:**

This review article appears to have been written at the same time that Seitz was working on his big book, his bible which laid down the solid state field. Were you aware of Seitz's effort at that time to write a book?

**Bardeen:**

I knew he was working on a book because we'd get together with him and if he had any spare time, he would put into writing on the book. He was the sort of person that never wasted any time, so if he had any free time, he would be writing away on the book. I knew he was writing the book but this article is a different sort of thing. This was not a treatise of the sort that he was writing, but just a review of the subject for the Journal of Applied Physics.

**Hoddeson:**

It indicates quite a bit of interest in this field to have a review article written in a journal like that.

**Bardeen:**

There had been a number of advances in understanding resistivity in metals and some of the work I did at Harvard was related to that. So this is written more from that point of view, starting at the beginning. So it's more an introductory sort of review but to try to bring it up to date with the most recent theoretical developments and then do an approximate comparison of theory and experiment.

**Hoddeson:**

There is an excellent bibliography at the end which I was particularly interested in. Also, I noticed that at the end, you point out three unsolved problems, the best known being superconductivity. Now very soon thereafter, you published an abstract on superconductivity.

**Bardeen:**

It is now known as the Kondo Effect which created a great deal of interest in the last 10 to 15 years particularly. What was the third one? That's the minimum in resistivity.

**Hoddeson:**

Semiconductors within completely filled D-bands.

**Bardeen:**

Oh yes. Mott pointed out a difficulty. If you just count valence electrons, semiconductors should be metals because they have incompletely filled bands according to the band theory, but they are not metallic but semiconductors or insulators. And so, this is an indication that the Bloch theory doesn't apply to these materials. This is another subject which has been developed. All three of these have been developed considerably in subsequent years.

**Hoddeson:**

What were you thinking about most deeply in that period?

**Bardeen:**

I was working on superconductivity primarily. The only thing published was just an abstract. I sent around a few preprints for comments. I wrote a paper and sent it around for comments. It looked like quantitatively, it was off at least by a factor of 10 or so. And so I never published the full paper. About that time I left to go to Washington to work for the Navy so that got stopped. But some of the ideas are carried over into the present theory, that there is a small energy gap covering the entire Fermi surface and that was the basis for this sort of a model. But the way the energy gap was obtained was different than it was at that time.

**Hoddeson:**

Was this a subject that lots of people were very interested in at the time.

**Bardeen:**

I sent around preprints to people who were interested like Seitz and others and got comments from them.

**Hoddeson:**

: I'm interested in what the ideas going around in your head were here that were so similar to the Peierls Distortion and Frohlich Mechanism later on.

**Bardeen:**

Well it was similar to that but I was trying to do it in regular three-dimensional model. All the same ideas were taken up by Frohlich for a simpler one-dimensional model, and there it apparently does work. But it doesn't seem to work in three-dimensions as quantitatively. I imagine there might be some particular cases where you'd get a Peierls sort of distortion in a three-dimensional model. It's known that you can in two-dimension, layer structures.

**Hoddeson:**

Well then the war came and you were brought to the U.S. Naval Ordnance Lab in Washington, DC, where you remained from 1941-45 as a civilian physicist. How did this assignment come through?



**Bardeen:**

Originally I was just going to be there a year -- that's before we were in the war. It was the summer of 1941.

**Hoddeson:**

What brought you down there?

**Bardeen:**

Lynn Rombauer who had been a professor at Minnesota went to Washington to work on this program. He had worked earlier at the Department of Terrestrial Magnetism in Washington. He was actually building a high energy accelerator that would bend terrestrial magnetism but they knew him. The problem was to protect ships from magnetic mines by so-called degaussing them, putting coils on the ships, running current through the coils to try to counteract the magnetic field of the ships so that wouldn't be seen by the mines. This is a subject which was worked on very intensively in Great Britain because they had difficulties with magnetic mines. You couldn't reduce the field of the ship to zero, you could just reduce the magnitude

which required them to make the mines more sensitive if the ship was going to trigger them. But that made them easier to sweep with mine sweepers. So it was a combination of degaussing and mine sweeping rather than degaussing alone which protected the ships. But that subject was getting pretty well completed by the time I was there and as the methods for doing this and the techniques were pretty well under way by the time I got there. So I got interested more in the other side of the problem. Well, one I was interested in was influencing fields of ships in general. I was later put in charge of a group which was charged with determining the magnitude of various influence fields of ships, not only magnetic but also acoustic and later for pressure mines. Again, one wouldn't use one of these effects by itself, say acoustic. One would use acoustic in combination again with magnetic to make it harder to sweep. You have to get the right combination.

**Hoddeson:**

Did you work in a team at this lab?

**Bardeen:**

I was in charge of a group which eventually got up to about 90 people, on influencing fields of ships. This extended not only to mines, but also to influencing firing of torpedoes.

**Hoddeson:**

I didn't realize you were a director at that time with so many people working for you.

**Bardeen:**

Lynn Rombauer was in charge of that section of a Laboratory and I was a division head under him.

**Hoddeson:**

Did you enjoy the role of the group leader?

**Bardeen:**

No, not particularly.

**Hoddeson:**

Were the people in the group Ph.D. physicists?

**Bardeen:**

A lot of them were. This background which was useful here went back to my Geophysics days. I recruited some of my geophysics friends to come. The sorts of work we were doing was very similar to the problems you run into in geophysics.

**Hoddeson:**

Did it relate at all to the work you were doing in solid state physics in any way?

**Bardeen:**

No, it was just classical physics and boundary value problems, things of that sort.

**Hoddeson:**

Just a general question about communications during the war: was there much communication between the Naval Ordnance Lab and other laboratories at that time doing similar work?

**Bardeen:**

We got into work on torpedoes, because of a difficulty they ran into, and it influenced firing the

torpedoes. They had magnetically fired torpedoes which were never really adequately tested by the Newport Torpedo Station, and when they took them out to the Pacific, they ran into trouble with premature firing. If the torpedo didn't behave right, if it popped out of the water or something like that, that triggered. They not only lost the torpedo but often gave away the position of the submarines. They turned off the magnetic firing device which would fire on impact. Then they found the impact firing device wouldn't work because I guess in the testing, they both were on, the impact firing device would work in combination with the magnetic firing device, but if they turned off the magnetic firing device, the impact firing device wouldn't work and so they had to make emergency repairs in the flux. And so you can imagine this caused a great amount of activity in the Naval Ordnance organization so they brought us in to see if we could help on the problem. I went up to the Naval Torpedo Station and talked with several people there involved in the program, and I made a number of trips up there. I also made trips to Westinghouse where they were involved in making torpedoes and in Seattle where they had some test stations for torpedoes. The

interesting thing in that period was the interview with Einstein who sent the idea to Naval Ordnance for a method for firing torpedoes, a non-impact method of firing torpedoes. They sent me up to interview him, which I did and had a very interesting talk with him in his study on the second floor of a house he lived in at Princeton. He described a method based on change of inductance of a coil essentially, when it passed under the seal hull of a ship. To do this, you require alternating currents and alternating currents are absorbed by the shell hull of the torpedo assembly. We thought about this possibility and got around this by using plastic heads but we didn't like to go into that. So it worked out very slowly. When I brought this objection to Einstein, he said we should build a plastic window on the lower head or use a plastic head. As a result of this, we took up the problem more intensively to see if we could design a warhead with a plastic window. And some months later, we found a German torpedo which had come up on a beach somewhere which had a firing device of exactly the same. And they had apparently been using them for sometime. So the Germans were using the design which Einstein proposed.

**Hoddeson:**

How did he happen to be working on this problem?

**Bardeen:**

I think he was a consultant for the Navy. Just how he got to be a consultant for the Navy I don't know. I don't know he came up on this idea, whether he got some hint of it through some German source or came on it independently. I don't know.

**Hoddeson:**

I wonder if you could make a general comment about the effects of the war on your own scientific work.

**Bardeen:**

I don't think it added anything to my own scientific work. It made use of the background I had picked up in my geophysics days but my main interest was in atomic physics and quantum physics and I didn't use that at all.

**Hoddeson:**

It's often said that the war had a large effect on solid state physics in general through the work on materials. I wonder if you would comment on that.

**Bardeen:**

Solid state certainly did in semiconductors using silicon detectors in radar. It also did in metallurgy.

**Hoddeson:**

Do you think those fields would have gone in different directions had the war not intervened?

**Bardeen:**

If the war hadn't intervened, there probably wouldn't have been the incentive to develop microwaves as rapidly, and develop the detectors for microwaves. Vacuum tubes couldn't be used so they had to go back to the old cats' whisker detectors for radio.

**Hoddeson:**

In articles I have read it's stated that the two reasons for choosing silicon and germanium were that first they were simple and second, because of the



tremendous amount of work that was done on them during the war. If that war work had not been done, do you think that these semiconductors would perhaps not have been chosen. Might they have stuck with the copper oxide and selenium?

**Bardeen:**

Before the war, he was working on silicon I think independently of the war effort so I think it would have gone ahead but perhaps a bit more slowly.

**Hoddeson:**

Then of course there wouldn't have been any interruption due to the war.

**Bardeen:**

I think the pressure was to try to develop an amplifying device using a semiconductor. I don't think the war had any direct effect on that. It probably slowed that down if anything because the people were all working on other subjects during the war. I think Seitz was working on semiconductors for radar detectors. I don't think he was thinking about amplifiers.

**Hoddeson:**

It's an interesting question, what impact did the war have on the field if any at all.

**Bardeen:**

It's hard to say. The direction which was followed was certainly different as a result of the war. We probably had better materials to work with due to the war efforts.

**Hoddeson:**

In what sense was the direction different?

**Bardeen:**

Well, in emphasizing detectors and looking for possible ways of getting gain, getting an active device.

**Hoddeson:**

Well in 1945, you chose to move on to Bell rather than to return to Minnesota. I'm interested in how you made this decision, what Fisk and Shockley's role was in hiring you at Bell, who recruited you and so on.

## **Bardeen:**

I think Kelly had been Director of Research and later President of the Bell Telephone Laboratories and had a great deal of influence in getting this program on solid state physics started at Bell Labs after the war. Even before the war ended, they were beginning to make plans to get people together. I think it was Kelly's idea to get together a group which had both chemists and physicists so that they could get the interaction between chemistry and physics and materials development. He also wanted some people who had some of the modern ideas of applying quantum concepts and trying to understand the properties of materials and attack materials on a rather broad front from the theoretical and atomic structure through preparation of materials like chemists and metallurgists. And this group was formed just to look into semiconductors but into all aspects of solid state. Magnetism was a big part of it. There had been a lot of activity in magnetism before the war and this was a major element. The group also looked into dielectric materials ferro-electric materials and all sorts of materials. And when I went there I didn't just go there to join the group to work on the theoretical aspects but didn't have any

particular branch in mind which I would concentrate on.

**Hoddeson:**

The story that you tell in some of your writings is that it was very crowded at Bell so they put you into an office with Brattain and Pearson and then you got involved in what they were doing. Is that the whole story?

**Bardeen:**

Just about the time the war ended in October or November 1945, Bell Labs brought in a lot of people for wartime research so the laboratories were overcrowded. This was Murray Hill. These wartime projects were still going on as the war had just ended; the turnover to peace-time activity was just beginning, so these people were still there. They were building a new building at Murray Hill which hadn't been completed yet. We were due to move into the new building when it was completed but at the time I moved there we were still in the old building in rather crowded conditions.

**Hoddeson:**

The old building in Murray Hill?

**Bardeen:**

Murray Hill.

**Hoddeson:**

By the way, was Murray Hill named after the tow or was the town named after the Labs. Do you have any idea?

**Bardeen:**

I think Murray Hill was there before the labs. It was the name of the area.

**Hoddeson:**

Do you know if Fisk or Shockley had any direct role in hiring you or was it mainly Kelly who...?

**Bardeen:**

Oh yes, very direct. I met Kelly but my main contacts were through Shockley and Fisk.

**Hoddeson:**

Who actually invited you?

**Bardeen:**

I guess my first contact was with Shockley, and then Fisk, and then eventually I talked with Kelly. I think Shockley and Fisk tried to get me interested in the job and it was Kelly who actually made me the offer.

**Hoddeson:**

How did they explain the position to you? Did they tell you who would be working on mission-oriented work?

**Bardeen:**

No, it was to be a basic research study in materials.

**Hoddeson:**

So you expected a free hand?

**Bardeen:**

Since they worked on materials from a rather broad front, it wasn't very restrictive. I could work on whatever theoretical problems I wanted to in

connection with materials, so from that point of view, it looked very good.

**Hoddeson:**

Was it a difficult decision to choose Bell over returning to Minnesota?

**Bardeen:**

From a financial viewpoint, I got about twice as much money at Bell Labs and really couldn't afford to go back to Minnesota for what they were offering me which wasn't much more than I was getting before the war. That was perhaps primarily because I was one of the first, probably the first, to return. Others returned later. I guess the authorities at Minnesota hadn't realized how things had changed. Tate, who was also involved in wartime research, and was really the prime figure at Minnesota arranged for an increased salary, and tried to get me to go back but the differential was too great. I'm not sure whether I would have gone back had it been even, but this looked like a very good opportunity at Bell Labs. I also enjoyed my associations at Minnesota. The major decision was made on economic grounds.

**Hoddeson:**

Very interesting. Was Bell in those days, considered to be a model of mission-oriented research, as it is now for example?

**Bardeen:**

I think it was always regarded as the prime industrial research laboratory and they do much more in the way of basic scientific research now which is not so directly related to missions. Of course, they did at that time. This was really an effort to get a research group started working on a broader range of things not directed toward any particular product but just to try to develop a field.

**Hoddeson:**

And yet, of course, all these problems had to be in some way tied to communications and materials which were being used in communications.

**Bardeen:**

Of course, materials were very widely used in communications which used all sorts of materials one way or another. So any advance you could make



on almost any aspect would be useful. There had been a good bit of work in materials before the war, but it hadn't been particularly tied into the modern atomic viewpoint of matter, that's the applications of quantum theory to understand the properties of matter. So, the research was more empirical rather than trying to understand the properties from a basic point of view.

**Hoddeson:**

Now this began to change at Bell. I've learned that there were some study efforts going on where people got together to educate themselves. Even people who hadn't been trained in quantum theory such as Pearson and Brattain came to these sessions.

**Bardeen:**

Apparently, this had been a tradition for a long time at Bell; to have small study groups working on this problem or this area or that area.

**Hoddeson:**

Do you recall participating in any of these when you first arrived?

**Bardeen:**

Yes. None of use had worked on semiconductors during the war and one of the first things we did was to find out what went on during the war, what progress had been made. And the papers of Shockley, which were published just toward the beginning of the war, 1940 or 1941, on the properties of rectifiers. We were just beginning to get hold of those and study those. They had a good bit of material on semi-conductors. Aside from that, we went through Pauling's book on the chemical bond which had come out around 1940, I imagine. L.P. Pauling, *The Nature of the Chemical Bond and the Structure of Molecules and Crystals* (Oxford, 1939).

**Hoddeson:**

Who was running the seminar? Who chose the books to study?

**Bardeen:**

It was just a group that got together and we would take turns leading the discussion.

**Hoddeson:**

How often did the group meet?

**Bardeen:**

At least once a week, sometimes twice a week.

**Hoddeson:**

Who else was in the group?

**Bardeen:**

The group working on semi-conductors -- Shockley, Walter Brattain, Gerald Pearson and I'm sure there were others there too, probably on the order of eight or ten together.

**Hoddeson:**

Alan Holden, Kittel perhaps?

**Bardeen:**

Alan Holden was there. Charlie Kittel? He came later.

**Hoddeson:**

Did Kelly ever come to these meetings?

**Bardeen:**

No. He may have in his earlier days. Earlier these courses were out-of-hours; people did the studying on their own time. Later the company allowed the courses on company time, so to speak.

**Hoddeson:**

Was the library fairly good in those days?

**Bardeen:**

I think it had a very good library.

**Hoddeson:**

Were there many visitors and visits to other places in that period?

**Bardeen:**

Well, we had visitors. We also had journal clubs and visitors giving talks, probably once a week or so. There was a good bit of activity. They also had consultants. Debye was a consultant.

**Hoddeson:**

How about Slater?

**Bardeen:**

I think he consulted on magnetrons and microwave devices, but not with our group. I think Debye is the main consultant that I remember in those days.

**Hoddeson:**

How did Debye interact with people? Did he give talks? Did he sit in a room and have people ask him questions? Did he run seminars? In what ways did he serve the researchers?

**Bardeen:**

We would tell him what problems we were working on and then try to get any suggestions he might have, what to do next or some literature he was familiar with and we weren't; he would refer us to that. Just get his general reactions on how we were proceeding on the problems.

**Hoddeson:**

Did he have a considerable impact on the quality of the research?

**Bardeen:**

I think he did at the start. Yes. But as we got into it more deeply, it was kind of outside of the field of his expertise. I think he was very valuable at the start.

**Hoddeson:**

How much more time do you have to spend on today's session?

**Bardeen:**

I should be leaving right now. I have a luncheon meeting I'm supposed to be at.

**Hoddeson:**

O.K. Let me save my next question for next time.  
Thank you

## Interview Session – 4

### **Hoddeson:**

This is Lillian Hoddeson and this is Session 4 of an oral history interview with Professor John Bardeen in his office at the University of Illinois. It's December 22, 1977. We left off last time soon after your arrival at Bell Labs in late 1945. To help refresh your memory of what Bell was like for you in those days, I brought along an organization charge, several in fact.

### **Bardeen:**

Was it 1945 or 1946?

### **Hoddeson:**

This chart is the July 1946 replacement of the January 1946 chart and on it we see the three new groups that had recently been formed under Wooldridge, Fisk and Shockley. For Bell, as I understand it, the formation of those groups was a departure.

**Bardeen:**

Yes, (looking at the Morgan-Shockley group), this is the new group which was formed after the war to work on solid state physics. I think I said earlier that Kelly and Fisk were probably the main instigators for this group, and Shockley played a big role in setting it up. It was Fisk and Shockley who got me interested in coming to the Labs, and according to this chart, was at that time in the group on semiconductors when I first arrived in late 1945 -- October or November 1945. I didn't know what field I'd be working in and as I think I said earlier, they put me in an office with Walter Brattain and Gerald Pearson and I started work on semiconductor problems. As you can see on this chart, I'm in the semiconductor group.

**Hoddeson:**

Were you aware of the master organization as reflected on this chart at the time? When they put you in that office, did that appear to you as though it was more or less accidental?



**Bardeen:**

This is July 1946 issue replacing issue January 1946, so at that time, this group was already formed. But I doubt if was formed in January, and it certainly wasn't when I first arrived. Because I looked over the activities of different people in magnetism and other areas and then decided to work on semiconductors.

**Hoddeson:**

Kelly had been talking, since the mid-thirties, about setting up a multi-disciplinary solid-state group.

**Bardeen:**

Yes. He wanted to get a group of both chemists and physicists involved.

**Hoddeson:**

Yes, he had been talking about that for years but he wasn't in a position to get it organized until this time.

**Bardeen:**

During the war of course, he couldn't, but right after the war, he got it started, and many of the people

were working on various wartime projects during the war including Walter Brattain and Gerald Pearson.

**Hoddeson:**

Yes. It was also through their work that Shockley got interested in some of his work on semiconductors before the war.

**Bardeen:**

Yes, he'd worked with Brattain on semiconductor problems before the war, but neither one worked on semiconductors during the war. But this is the group set-up here, which is essentially the same group we had when we discovered the point-contact transistor. About the only additional people involved were those in Goucher's group, who were then transferred to work on semiconductor problems; the group under Shockley was augmented by two groups under Goucher.

**Hoddeson:**

Did you work closely with Goucher's group during the transistor work?

**Bardeen:**

I didn't, but Bill Shockley did. There was the famous Haynes-Shockley experiment. Some of these were Technical Assistants, like Ryder and Fois. (J.R. Haynes and W. Shockley, Phys. Rev. 2. 691 (1949)].

**Hoddeson:**

And Griffith?

**Bardeen:**

I guess he much have been a technical assistant. Gibney was a chemist; Moore was an electrical engineer. I see Townes is listed in this group but I'm sure he didn't stay there very long because he was in a different area, not in solid-state physics.

**Hoddeson:**

Well, he worked under Morgan.

**Bardeen:**

Yes, he's listed under Morgan. But, I think he was working with a different group at the time, not in this but the solid-state physics group. I'm not sure which one.

**Hoddeson:**

I wish I had the previous chart which reflects the war organization. On the very next chart, Townes is still under Morgan, and then in 1948, I don't see Townes at all.

**Bardeen:**

1948 was, I think, when I worked with him. He was working on microwave problems, the spectra of gasses.

**Hoddeson:**

Perhaps we should take a minutes to discuss the work that you did with Townes and we will be able to focus entirely on the transistor. I'm interested in how you got involved with this work and how it related to other problems at Bell.

**Bardeen:**

Townes came around with the problem which he had been working on, the spectra of molecules, and he could make very accurate measurements, and the problem was to calculate the effect of quadrupole moments and hyper-fine splittings. I worked out the

theory of the effect, and some of then complicated looking formulas here that just involve quantum numbers and we could calculate the position of the lines very accurately to many significant figures. We had a real feeling that quantum mechanics was correct since we could calculate the position of all these lines to many significant figures.

**Hoddeson:**

What did this have to do with Bell Laboratories' interests?

**Bardeen:**

I think he got interested in this because he worked on microwaves during the war and then this was an application of measuring the microwave absorption spectra of gasses. We could measure the lines very accurately and he noted that you could determine the nuclear quadrupole but he needed a theory to work out and analyze the experiments. And, I worked out the theory.

**Hoddeson:**

And the Bell Labs' research group was set up in such a way that he could just walk in and way, "Well,

Bardeen, I need some help with this calculation. Do you think you can find the time?" That was perfectly O.K.?

**Bardeen:**

Oh sure.

**Hoddeson:**

You didn't have to go to Shockley or anyone?

**Bardeen:**

No, no third party.

**Hoddeson:**

I am interested in the way in which the solid-state group was set up. It was set up as an interdisciplinary group to solid-state physics divided up into magnetism, crystals, die electrics, semiconductors. I was wondering whether the organizational attempt to put people working on different aspects of solid-state physics all together in a formal group in practice actually led to interdisciplinary work.

**Bardeen:**

Well I think it did. We worked very closely together at the seminars where we reviewed earlier work.

**Hoddeson:**

Are you talking about the weekly solid-state seminars?

**Bardeen:**

Yes, all of these people would attend the seminars. Well not all, but those interested would. We had close contacts with Gibney for example. He was a chemist and we interchanged ideas with them, and also with other people in chemistry and metallurgy who weren't in this group. We had close contacts with them too.

**Hoddeson:**

You're suggesting that the main interaction among the disciplines came in the seminar discussions.

**Bardeen:**

Or just in informal discussions at any time, discussions at lunch for example.

**Hoddeson:**

Were all these sub-groups of the solid-state group physically working in the same general location. Was everyone in the Morgan/Shockley group at Murray Hill?

**Bardeen:**

Townes was not. I don't think he was in this group very long. His office was in a different place with his laboratory.

**Hoddeson:**

I see. Was Kelly around a lot? Was he sort of in the background and watching these interactions going on?

**Bardeen:**

We'd see him occasionally, but not too frequently.

**Hoddeson:**

Would he talk to you about your work?

**Bardeen:**

No. We talked with him about the work indirectly.



**Hoddeson:**

The mythology is that he was the organizational mastermind behind this solid-state group and I suspect that certainly true in part. I was wondering how he functioned.

**Bardeen:**

He was too high up in the organization to worry about people down at the bottom. He was certainly instrumental in setting it up, and I'm sure followed what went on through the reports and things like that but not directly. Only when we had something significant to show him, then he might come around to visit occasionally, but not very frequently.

**Hoddeson:**

Let's stay with the seminars for a minute. Now Herring recalls several seminars: a solid-state seminar, and then a similar seminar that his group attended, let's see, he isn't on this chart yet but he will be on the next chart; he appears in the Wooldridge group which was later taken over by Addison White. Herring says they went through a similar seminar going through DeBoer's (J.H.

DeBoer, Electron Emission and Absorption Phenomena (N.Y. Macmillan, 1935)] book on the electronic processes in solids just as the solid-state group was going through Mott & Gurney's [N.F. Mott and R.W. Gurney, Electronic Process in Ionic Crystals (Oxford, 1950)] text, and others.

**Bardeen:**

Well, we went through a number of texts and the people involved would be different. I was interested in semiconductors and went through the work done during the way which was just published at that time by Torrey & Whitmer H.C. Torrey and C.A., Whitmer, Crystal Rectifiers (McGraw Hill, 1948)].

**Hoddeson:**

Did you go through Torrey & Whitmer's book in the seminars?

**Bardeen:**

No, not the whole book in detail but the parts we were interested in. And then we went through Shottky's papers which were published just before the war.

**Hoddeson:**

Had you, by the way, seen them before the war?

**Bardeen:**

No.

**Hoddeson:**

Shockley had of course.

**Bardeen:**

Some later ones were published in 1941. I'm not sure whether we had access to them or not.

**Hoddeson:**

Shockley I know saw the earlier papers; I think there was a 1939 paper.

**Bardeen:**

Yes the earlier papers, he was familiar with those by Mott and Shottky on the rectification. But there were later papers on the metal semiconductors rectifier in which Shottky went into theory in much greater depth, some written with an associate of his, Spenke. That's one of the things we went through; we were

trying to get caught up in what was going on in semiconductors. As you say, we went through Mott & Guerney and Pauling's book on the nature of the chemical bond. And I've forgotten what the other books were.

**Hoddeson:**

Who chose the text, or the papers to be reviewed?

**Bardeen:**

Just general agreement, if we had enough people together interested in going through them or participating in seminars. It was very informal; we just got together and did it. As I recall, at first they were out-of-hours. We got together at five o'clock after the official quitting time. Then later, we did it in-hours.

**Hoddeson:**

Were they once a week meetings?

**Bardeen:**

At least once a week, sometimes more frequently. I think in the semiconductor work it was more frequent.

**Hoddeson:**

I see. And this was in addition to the regular colloquia. Now the Journal Club began at some point also, was that later? I have not been able to find out when the Journal Club began.

**Bardeen:**

I don't remember now.

**Hoddeson:**

Herring has some records but they begin in the fifties.

**Bardeen:**

I'm sure it began before that, just when I don't know.

**Hoddeson:**

And then Herring remembers some formal lecture series in quantum mechanics and statistical mechanics. Did you attend those? They may have been on a more elementary level.

**Bardeen:**

No, I didn't go to any of those.

**Hoddeson:**

The main series then was this solid-state series, the informal going through papers and texts that you were involved in?

**Bardeen:**

Yes, there was that and then the Journal Club in which we'd review the current literature. We even got a group together to study Russian, to learn to read it anyway.

**Hoddeson:**

Before we leave the seminar, I'd like to ask another question. The people who were involved had very different backgrounds: Brattain had essentially no quantum mechanics, Gibney is a chemist, and so on. The general level of the seminar must have been very variable because the people who were involved were so different in their training.

**Bardeen:**

Well it would depend. For example, going through Pauling's book was right up Gibney's alley and Mott and Gurney also did a good bit of chemistry. We did

more chemistry than quantum mechanics; we used essential ideas from quantum mechanics but not detailed theory. I think that was true of Schottky's papers too, once you accept the basic ideas of electrons and holes, it's essentially classical ideas. The things we talked about in the seminar didn't require any deep knowledge of quantum theory.

**Hoddeson:**

Did you also discuss the reports that were coming out of the Purdue group? For example, on germanium?

**Bardeen:**

Yes.

**Hoddeson:**

They were available?

**Bardeen:**

They were available. I don't know how much of it we went through in the seminar. I know they were available. Lark-Horovitz and Johnson looked at the transport properties of semiconductors in considerable detail and went through those papers. I

think they were published just after the war or in the years just following the war. We also had the wartime reports available. So we were pretty well up on semiconductors. As I said, it was a new field for me, so I was learning from any source I could use.

**Hoddeson:**

You never read this stuff before the war then? Mott's theory of rectification for example, had you read that before the war?

**Bardeen:**

I read that before the war, but it was just general reading and nothing else. I had no interest in working on that on my own. But Schottky's work was new, that's true, later papers were new.

**Hoddeson:**

Were there any other important pieces of work going on besides the work of people like Schottky, Mott and Davidoff who were writing theoretical papers and experimental work such as by the Purdue group? I'm trying to learn what were the main pockets of work that people were trying to learn at that time to gain a general background.



**Bardeen:**

Well, I would say Torrey and Whitmer, which told what they did at the Radiation Lab during the war in semiconductors.

**Hoddeson:**

Did they do much?

**Bardeen:**

They did a good bit involving silicon for detectors and we went through theory of rectification. I think Esther Conwell and Weisskopf did that. Some theory of scattering. There was good bit of work done during the war which we took advantage of. And Schottky's work and then during the war, the concepts of doping were rather vague. It was thought that you could dope with three to five compounds and get p-type, but it hadn't been proven, say that a group five element really did go in such extra electron could be detached from the donor and make the semiconduction in type. It's one of the things Gerald Pearson was working on, to try to pin down these ideas more definitely, and he made a good many measurements on transport properties. He and

I wrote a joint paper which was published in 1949, I think.

**Hoddeson:**

Oh yes, that beautiful silicon paper. I have that with me.

**Bardeen:**

It was a very popular paper after the transistor came out for people who wanted to get into the field. I've heard that Bell Labs issued something like 5,000 reprints, so that paper probably got distributed more widely than any other that I've written.

**Hoddeson:**

It has a very beautiful balance of theory and experimentation.

**Bardeen:**

Most of the ideas came from earlier work. This was really putting them all together and showing quantitatively how things worked out.

**Hoddeson:**

Let's see, during the war you mentioned the theoretical work, work of the Rad Lab, work, of the Purdue group and Bell. Now there was some work done at Penn, wasn't there?

**Bardeen:**

There was some work done at Penn. We had all those reports available to us. The work at Bell was mostly in metallurgy and materials. One was working at Holmdel. He was one of the first to suggest using silicon as a detector for microwaves and he did experiments on silicon. Brattain was familiar with that work and some of the data before the war.

**Hoddeson:**

Did you interact much with Ohl?

**Bardeen:**

He went down and talked with him at Holmdel on quite a few occasions.

**Hoddeson:**

He seem to have been fairly close to something like a transistor himself. Do you feel that is true?

**Bardeen:**

He observed some of these optical effects, photo-effects which suggested that there was an inversion layer on silicon.

**Hoddeson:**

Apparently, he built a radio without using vacuum tubes, using negative resistance elements, and that was demonstrated just after the war to Shockley. Shockley mentioned it in one of his articles. Does that ring a bell?

**Bardeen:**

I don't remember that it is quite possible. Using thermistors?

**Hoddeson:**

Yes, I think so.

**Bardeen:**

Using a diode for detector.

**Hoddeson:**

Yes, but it was apparently very unstable. I left a strong impression on Shockley. I don't have any documents to back that up.

**Bardeen:**

He was a sort of intuitive experimenter. He didn't have any great knowledge of theory but he did these experiments.

**Hoddeson:**

In what language did you two speak to each other? Ohl didn't know quantum mechanics. I gather he used a sort of chemical language.

**Bardeen:**

We just talked about the experiments. Just talked in terms of what experiments he was doing. The ideas of n- and p-type or excess and defect, started being used during the war. We talked about it in a natural language. He was able to prepare a surface such that

it had an inversion layer on the surface which Brattain remembered. That's one reason we started with silicon rather than germanium. The first electrolytic transistor was with silicon. That was the reason for picking that particular combination. We had some evidence from Ohl's work that there was this inversion in silicon and we made use of the inversion layer.

**Hoddeson:**

Did you understand that that was an inversion layer when you first began working with silicon?

**Bardeen:**

Well, I suppose we're getting a little bit ahead of the game, but when Brattain and Gibney found a change in contact potential with light and that you could move the surface barrier up and down by applying an electric field through an electrolyte, they thought that with their experiments, as I remember, were with both silicon and germanium. The idea of using a point contact geometry was mine, to try to make an amplifier. And the idea for doing that was to try to get away from thin films which we had been using before but to use bulk materials. This experiment

they were doing of course used bulk materials, and bulk material has much better electrical properties, much higher mobility, so that part of this was just a simple way of testing the idea.

**Hoddeson:**

I have a question here. In looking at the series of experiments that led to the final contact transistor, I see you started out with the field fact and then the effect of moisture led to replacing the electrolyte with deposited thin oxide film and then...

**Bardeen:**

It was just trying to observe the field effect.

**Hoddeson:**

It was only at the end and this is the question I have -- it seems almost to have been by accident that you went to a point contact effect....

**Bardeen:**

We went to the point contact to observe the field effect and that was why we insulated the point from the electrolyte. We applied the field across the electrode.

**Hoddeson:**

At what point did you realize that holes were coming through?

**Bardeen:**

Well, that was later. Experiments worked with the electrolyte but we wanted to get rid of the electrolyte and tried to do that by forming an oxide and then evaporated gold on the oxide, and then put a point contact across to observe the field effect. We found an effect but it was in the opposite direction than you'd expect from the field effect which showed something else was going on. And, that's when we thought this must be injecting holes.

**Hoddeson:**

That late along in the series?

**Bardeen:**

We just found it experimentally first and it was just a small effect. The gold contact wasn't insulated from the germanium as the thin layer of oxide on the surface wasn't enough to insulate it. It's like the geometry of the present MOS transistors. This



wasn't discovered until I guess the early sixties -- how do we make a good insulated oxide so you can make MOS transistors.

**Hoddeson:**

The turn in this series of experiments that led to a transistor seems to me to have occurred when you noticed that water was interfering with an early experiment which Brattain conducted just after your surface state theory came out.

**Bardeen:**

After the surface state suggestion, Brattain started work on surface problems and Pearson was involved in studying bulk properties.

**Hoddeson:**

But in the process of studying the temperature dependence of the contact potential, Brattain noticed a hysteresis effect due to water. And, that's what led to immersing the apparatus in an electrolyte. This is according to Brattain's description. I mean I'm getting this from his description.

**Bardeen:**

I don't remember details of that.

**Hoddeson:**

It struck me that while they were just trying to get rid of the hysteresis at first, in fact the water was doing something very important. I don't know if that was recognized at that time.

**Bardeen:**

I don't remember. There was the discovery that you could do measurements of the change of contact potential with light -- that you could move the surface barrier up and down with an electrolyte which showed that you could get around the surface states if you applied a strong enough field through the electrolyte so that you could get by the surface stage.

**Hoddeson:**

The question is what did you think was happening with the electrolyte? What was enabling one of get through the surface states?

## **Bardeen:**

The ions were very close to the surface and there was a strong field at the surface. Practically all the voltage drop -- the voltage to the electrolyte -- would occur right across the interface of the electrolyte and the semiconductor. So you get a much stronger field as you are applying the voltage over atomic-like dimensions than over finite distance. And so we thought that the reason why it was so successful was that initially we had a lot stronger field at the interface but no doubt the interface itself, the surface itself, was changed since the electrolyte dissolves the surface. It dissolved whatever oxide was there.

## **Hoddeson:**

I'd like to backtrack a little bit. We left out a few things. In looking at what was going on at Bell in the thirties, before you arrived there, it seemed to me there were three major threads that were coming together at the time of the war. The whole thing was interrupted of course by the war. First was the work that was being done by Ohl along with the metallurgists on silicon. Second was the work of Brattain, Pearson and others on the various "istors"...

**Bardeen:**

Ohl's work was stimulated by trying to get a detector for microwaves which would work in the microwave region. The work on microwaves started independently at Bell when they were just trying to punch the radio frequency range out into the microwave range.

**Hoddeson:**

That's also what got Bell involved in radar. Isn't it? The fact that they already had people working on microwave frequencies.

**Bardeen:**

Yes, the fact that they already had people working in this area was one reason they got into the radar problem.

**Hoddeson:**

The third line centered about Shockley, Nix and Wooldridge -- the new group that was put together and given an unprecedented amount of freedom to investigate problems from a very fundamental point of view.

**Bardeen:**

I think that one of Kelly's ideas in hiring Shockley was to get some modern ideas on solids introduced into the Lab.

**Hoddeson:**

Well, apparently Shockley was introduced by Kelly to the idea of the need for a solid-state amplifier very early in 1936. Kelly spoke to him about that need. And, Bell didn't know very much about how to go about getting such an amplifier.

**Bardeen:**

I don't think there was any concerted effort to try to do it. People were stimulated to thinking about possible ideas and how it might be done but there was no program set up to do it.

**Hoddeson:**

They all did this business with trying to put a grid in a semiconductor. Shockley tried it. Brattain and Becker tried. They did a calculation which showed that it was unlikely that they could succeed, but they were certainly thinking along those lines. Shockley

also attempted to make an amplifier with Holden employing the piezoelectric effect. It didn't work.

**Bardeen:**

They tried quite a few things which didn't work before the war. Just trying to do something analogous to a vacuum tube by putting the grid in the space charge layer in the rectifier -- the metal semiconductor rectifier -- but the dimensions were too small.

**Hoddeson:**

Did you do something along these lines?

**Bardeen:**

No, it was Shockley and Brattain. As I understand it, Shockley would come up with these ideas and Brattain would try them out even though he wasn't very optimistic about the results [laughter]. And he tried out a number of things but none of them worked.

**Hoddeson:**

Is there anything more we need to say about the studies that you and the other members of the

semiconductor group participated in right after the war. I guess everybody studied Wilson (A.H. Wilson, Proc. Roy. Soc. (London) A 133, 458 (1931); A 134, 277 (1931)]. That was a classic paper. By the way, when did you first meet Wilson. I'm just curious.

**Bardeen:**

I don't remember. I don't think I ever met him before the war in this country. I think he went into business of some sort after the mid-thirties and was no longer involved in science.

**Hoddeson:**

Was his 1931 theory of semiconductors very widely read?

**Bardeen:**

Yes, his work kind of set the stage for understanding in terms of excess and defect electrons and holes so this is one of the basic papers, and Frenkel papers.

**Hoddeson:**

Yes, on photo-conductive phenomena...

## **Bardeen:**

... explanation of the change in contact potential with light, which really involves all the present ideas which were required for the transistor. If you generate electrons and holes at the surface, and the diffusion coefficient is different, so that the electrons tend to drift faster than the holes by diffusion, then the electric field has to be set up to equalize the flow of electrons and holes, so there's a change in the surface potential which can be observed as a change in the contact potential with light. So, this involved the ideas of diffusion and mobility, flow in an electric field, and compensation of electrons and holes and also had non-equilibrium conditions in that you had excess electrons and holes introduced by light. And the equations used to analyze these experiments were true in the early thirties and everything needed to analyze the junction transistor. All the basic equations were there.

## **Hoddeson:**

When did you first read Frenkel's paper?



**Bardeen:**

I think it was in this period after the war that I read these papers.

**Hoddeson:**

Were they readily available in this country?

**Bardeen:**

Yes, probably I had to get a translator from the Russians. They were among the classic papers. Before the war, Brattain had been concerned with copper oxide rectifiers and he'd had some data in his notebook on the oxidation of copper, which he analyzed in terms of the theories developed by Wagner and Schottky concerning the defects, vacancies and interstitials in the oxide? But in this case, it was necessary to understand what was happening during the drill and this is one of the first papers which we wrote after the war and I think it's the only paper ever written with all three of our names on it.

**Hoddeson:**

Is it?

**Bardeen:**

I think it's the only one with the three names.

**Hoddeson:**

This is the Journal of Chemical Physics Vol. 14 paper [J. Chem. Phys. 14, 714 (1946)]. Well let's see how Brattain's notebooks were filled with observations. Was he aware of the theory of Frenkel and others....

**Bardeen:**

I'm not sure of how much he was aware of the theories. This is a period in which--there is a reference to Mott and Gurney which is one of the things we looked at. So I'm sure we studied this sort of thing in our seminars. The basic sort of theory of vacancies and interstitials, in this case copper ion vacancies.

**Hoddeson:**

According to note #9 in the article, you did the theoretical interpretation and the experimental work was done by Bardeen and Shockley.

**Bardeen:**

Well Shockley's name got put on it mainly because he was director of the group and we consulted him occasionally about it, but he really didn't have a whole lot to do with it. It was mainly my talking with Walter Brattain about his early data and then developing the theory of it.

**Hoddeson:**

Was this done before the decision to study silicon and germanium?

**Bardeen:**

No, we went very early, even before that, to concentrate on silicon and germanium but it takes time to set up and get experimental data. And this data was available, so I decided to work on this. It involves, as you can see in this case for ions, the same sort of equations which are now used to discuss the flow of electrons and holes in semiconductors. The equation of motion for defects in semiconductors, ionic crystals, are very similar to sorts of theory required for electrons and holes. The theory as regards defects goes back to Wagner and

Shottky and we discuss here Wagner's theory of oxidation.

**Hoddeson:**

Is Wagner one of the leading people whose papers an historian should be studying?

**Bardeen:**

Yes, he did a lot of the pioneering work. Siemens Company in Germany gave money for a Shottky's prize in physics a few years ago. They honored Shottky on that occasion. I guess he was probably past 85 then. He was honored then at a symposium. I gave a talk in connection with the introduction of the prize and Wagner was present on this occasion. I reviewed the importance of Shottky's work. And Shottky, as I understand, was reluctant to have the prize named in his honor, because he thought that Wagner didn't get enough of the credit for the idea of Shottky defect. He felt that more credit should go to Wagner and he was apparently reluctant to have the prize named in his honor because he felt so indebted to Wagner for many of the ideas. Wagner is certainly one of the key people. I'd say the other key people were Wilson, Mott, Shottky and Frenkel.

**Hoddeson:**

And what about Davidov?

**Bardeen:**

I wouldn't say his ideas were so basic. He was trying to understand the pn-junction and wrote down some of the equations but he didn't use the right boundary conditions so he didn't get the transistor effect.

**Hoddeson:**

Was there work going on in European experimental laboratories as well? Most of the theoretical work was done in Europe before the war.

**Bardeen:**

In this field most of the theoretical work was done in Europe, although Wagner was at MIT. I'm not sure when he came there. He was a professor at MIT for many years. Before the war, work on semiconductors was done at industrial laboratories and was closely tied with applications.

**Hoddeson:**

Was any experimental work done in Germany and England on semiconductors. The theory was coming out of those countries.

**Bardeen:**

There was more done in Europe on semiconductors before the war. But there was not a very close correspondence between theory and experiment, mainly because the materials people studied were copper oxide and selenium which were useful in making rectifiers or for photo effects and these materials are hard to prepare and control the composition, and so they weren't very well suited for fundamental studies. So these ideas we've been talking about are mainly used to get a qualitative understanding out of the quantitative various phenomena. It wasn't until 1939-1940 that rectification was understood. At one time it was thought to be a tunneling effect.

**Hoddeson:**

You mentioned to me once that you took a trip with Shockley in 1947....

**Bardeen:**

Summer of 1947.

**Hoddeson:**

You visited some of the European labs. I was wondering what you learned, particularly whether they were doing anything like what we were doing here on semiconductors.

**Bardeen:**

We were interested in solid state physics in general.

**Hoddeson:**

Did Bell send you?

**Bardeen:**

Yes, Bell sent us to find out what was going on.

**Hoddeson:**

Was Kelly involved in sending you?

**Bardeen:**

I think he had something to do with it. He may have instigated it. It was a very interesting trip. It was not

long before that Bell reached some sort of agreement with Phillips which allowed us to visit Phillips Labs. I'm not quite sure just what that agreement was, some patent exchange or something of that sort. We were welcomed in the Phillips Labs and we visited Mott at Bristol.

**Hoddeson:**

Did you discuss rectification with them?

**Bardeen:**

Yes. I'd done some work on frequency dependence of the impedance of a metal-semiconductor rectifier and talked about that with Mott and some of his students.

**Hoddeson:**

And what else did you learn?

**Bardeen:**

I think Mott, at that time, was more interested in plastic properties of metal, dislocations, and things of that sort. Dislocations hadn't been seen at that time, but we were shown experiments of Runier in Paris which were suggestive of dislocations. I'd have



to look up my notes on the trip to find out the details but the highlights I remember. We visited Mott in Bristol and came back on the train to London with Sasnowski who had been picked up by the British just after the war from a concentration camp brought to England and he was working in England at that time but, then he eventually went back to Poland to work on semiconductors. He was head of an institute there. We visited Cambridge. I think we visited Oxford and then we went to Holland and visited Phillips, Casimir and....

**Hoddeson:**

Were they doing semiconductor work in Holland at that time?

**Bardeen:**

Yes they were doing very interesting work, not on germanium and silicon but on things like cadmium sulfide which are difficult to dope because you can introduce defects very easily into them. In general, if you try to dope, the charge is compensated by electrons and holes, but by the charge in the defects. They did very nice work on showing how the defects compensate the charge and the equilibrium equations

which determine that. And then also at that time, they were working on ferrites, and just beginning to get some good material for ferromagnetic insulating. They had very good people working on that project. We went to Paris and met Neel and we talked about ideas for which he later won a Nobel Prize on anti-ferromagnetics and how you could make permanent magnets by having very small particles so that you only have one domain for each particle and you have a very high coercive force. It was a very hot day in Paris. I think it set the all-time records for Paris on that year for that day of the month; it was a very hot day. We sat at an outdoor cafe and talked with Neel about these problems. We spoke to him in our high school French since he didn't want to speak English. It was a very interesting conversation because he was full of all these ideas about magnetism. And then we went to Zurich where Busch had been working on semiconductor problems. This was one of the few places which had been working specifically on semiconductors.

**Hoddeson:**

What was he doing?

**Bardeen:**

He was studying transport properties and the properties of semiconductors. I've forgotten now what materials he was working with but, as I recall, it was not silicon and germanium but other compound semiconductors. He had a very active experimental program. We didn't go to Germany because conditions were still pretty unsettled there. We went to Germany later -- met Shottky and Spence.

**Hoddeson:**

When?

**Bardeen:**

I think that was 1953.

**Hoddeson:**

Oh, much later. You mentioned earlier that in Europe, theoretical and experimental work was separated. Was the experimental work in the United States by comparison more closely integrated with theory?

**Bardeen:**

Typically they had different institutes with a professor in charge of the institute and very little interaction between the different institutes. In Göttingen for example, this was on a later visit with Hilsch and Pohl, who did very fine work before the war on ionic crystals.

**Hoddeson:**

They actually succeeded in placing the grid into an alkali?

**Bardeen:**

Yes, for a very low frequency. At that time, Hilsch was interested in thin film superconductors at low temperatures. Pohl had retired but still carrying out some research on ionic crystals. Pohl was a typical professor type. When we talked with him about his work, and something came up he couldn't answer, he'd call in his assistant from another room and he would come in to answer the question and then he would go out again. He was a person with a lot of charm. He didn't know much theory but he had good intuition about what sort of experiments to do. The experimental group was on one floor of the building and on the floor above was the theory group but I

don't think the theory group paid any attention to what the people on the lower floor were doing. There wasn't any interaction at all as far as I could see., But that's very typical I think. People in a theoretical institute work on their own problems and don't pay much attention to what is going on in the other institutes. That wasn't true of Mott. I think Mott was always very involved in experiments. Mott worked with Frank in crystals growth and things of that sort. That was when Frank was doing work with crystals experimentally. Mott was much involved with it.

### **Hoddeson:**

The flurry of development in Europe from about 1928 to 1932 or 1933 which resulted in the quantum theory of solids seems to have settled down by 1933 with the exception of the group around Mott in England where there was still quite a bit of work going on. Is that an oversimplified approximation?

### **Bardeen:**

I think that's true. Just after the quantum theory was discovered they were applying it to anything they could think of applying it to, and the theory of solids

is one of them. Then the neutron was discovered in 1932 and so nuclear physics became a very popular field and a lot of the theorists switched their attention. Only a few remained working on solid state problems. I think all of the leading theorists in that period from 1928 to 1932 at one time or another worked on problems which we now call solid state. In the late 30's, very few were working on solid state theory.

**Hoddeson:**

Mott was an exception. Let's see, is there anything else that was of interest about your trip to Europe?

**Bardeen:**

I found it very stimulating and picked up many new ideas. It was a very profitable trip. And I met people whose papers I had read and had admired but never met before. I guess through the prestige of the Bell Labs, they arranged so we could see their leading work. Physics was pretty much removed from Paris during the war because of the German occupation and Neel and his coworkers were at Grenoble, but he came up to Paris and met us there. We had a very

fine reception at the Phillips Labs and met Mott.  
Also at Cambridge.

**Hoddeson:**

Was there any work going on in Scandinavia? I notice a reference to Benedicks in one of your papers of 1915 work on germanium and silicon.

**Bardeen:**

That was very early work. The first publication of work on germanium I think was by Benedicks.

**Hoddeson:**

I think there was a reference in your Bell lectures. [See p. 79 of J. Bardeen, "Semiconductor Research Leading to the Point Contact Transistor," *Lex Prix Nobel en 1956* (Stockholm, 1957)].

**Bardeen:**

Not much was done until Lark-Horovitz picked it up during the war. He was the one mainly responsible for the activity in this country on germanium during the war. He never made a useful rectifier for microwaves, silicon was better, but work they did

with germanium was very important for our development.

**Hoddeson:**

When was the spreading resistance work done. Was that work done right after the war?

**Bardeen:**

That's Bray's work. I think that was just after the war. It was an outgrowth of the war-time activity.

**Hoddeson:**

Were you aware of the details of that study? By the time the war was over, I gather there wasn't quite as much information exchanged. Or was that not true? I got the impression that during the war, information was exchanged freely among the people who were working on common problems. Is that not true?

**Bardeen:**

Well, I wasn't involved in the activity during the war but after the war at meetings people talked. But of course travel was more difficult than it is now and we couldn't get together as frequently as we do these days.



**Hoddeson:**

I understand there was work going on at Sperry and RCA. Or was that relatively unimportant as compared to the work at Purdue?

**Bardeen:**

There were a lot of arguments in regard to patents for example with regard to doping. I've forgotten whose patent it was. Now who claimed credit for doping the 3-5 compounds. But I don't think his patent held up. They argued about it in the courts for a long time. I wasn't familiar with that work. I wasn't as familiar with that work as I don't think they published very much and we depended greatly on the reports which were published during the war, from the Radiation Lab, and from Purdue, and from the Pennsylvania groups. I think places like RCA and other places didn't publish very much and I wasn't familiar with what they did.

**Hoddeson:**

I think we're up to a long discussion on the transistor which starts with Shockley's design for a field effect amplifier. Shall we continue next time?

**Bardeen:**

I think that would be best.

**Hoddeson:**

Thank you again.

## Interview Session – 5

**Hoddeson:**

Last time, we left off just at the beginning of the discussion of the work of the semi-conductor group. Soon after you started working in this group, along with Brattain and Pearson, you turned to consider copper oxide film...

**Bardeen:**

Well, we weren't studying copper oxide films, but Walter had some data on copper oxide dating from before the war, which he showed me. And it was through trying to understand that data, that we worked out the theory for the oxidation of copper. What he was measuring was the rate of growth of the oxide on copper at different temperatures., And I was able to interpret this in terms of a mechanism, as I recall, involving a vacancy, motion in the oxide layer.

**Hoddeson:**

How did you move from that work to the work on silicon and germanium?

**Bardeen:**

The main activity was on silicon and germanium, but of course it took a while to get organized and start getting data, and the data on copper oxide was already available, so...

**Hoddeson:**

So that the work on copper oxide was done after the decision to focus on silicon and germanium had already been made?

**Bardeen:**

Yes, it was just kind of a sidetrack. But it did introduce some of the ideas on diffusion, the way diffusion takes place and things of that sort. The paper is the only one with our three names on it which I know of.

**Hoddeson:**

You commented very briefly on that paper last time and I think you mentioned that actually Shockley didn't do terribly much on this.

**Bardeen:**

We had a little discussion as to whether we should have his name on it. Since he was head of the group, he decided to put his name on it. He didn't really have a whole lot to do with it.

**Hoddeson:**

At this time, this is very early on, was the group already focussed on finding a semi-conductor amplifier?

**Bardeen:**

It was something just in the back of our minds. The research program was directed toward understanding the properties of semiconductors. The only important aspect of the work was Shockley's ideas on the field effect transistor, which was tried out by several people without success. That led to the surface states paper.

**Hoddeson:**

Now, Shockley had already done some work with semiconductor amplifiers before the war. He built

one using the piezoelectric effect and then a copper oxide rectifier...

**Bardeen:**

I was just looking at his paper on the history of the transistor (W. Shockley, "The Path to the Conception of the Junction Transistor," IEEE Trans. on Electron Devices, Vol. ED-

**Hoddeson:**

Which one of them?

**Bardeen:**

IEEE transactions...

**Hoddeson:**

I've seen it, that's an excellent paper, I think --

**Bardeen:**

Well, he put in a lot of research, went to the old notebooks. I think it's pretty good except for a few minor points, I might have some disagreement with him. On the whole I think he did a good job of reconstructing the history.

**Hoddeson:**

Well, let's go through it step by step. I'm particularly interested in places where you disagree with his interpretation. Before we get involved, I want to ask one question about Kelly. Was he aware of the details of what you were doing in the early stages of the work?

**Bardeen:**

No.

**Hoddeson:**

No, he wasn't. He

**Bardeen:**

... he set up the group and got it organized, but he didn't follow in any detail what we were doing.

**Hoddeson:**

And did the semiconductor subgroup of the solid state group interact very much with the other members of the group who were not working on semiconductors... Morgan...

**Bardeen:**

... oh yes, everyone interacted ...

**Hoddeson:**

... I mean with the Morgan subgroup, Bozorth's group the Goucher group, and Mason's group. I'm wondering what the interactions between those subgroups were like.

**Bardeen:**

Well, it varies between the different groups. Mason's group, with the exception perhaps of Walter Bond, didn't interact too strongly with the other groups. But all of the other groups interacted.

**Hoddeson:**

Were you aware of the details of their experiments and vice versa? Was there much input from other individuals?

**Bardeen:**

These study section would have people from all these different groups.

**Hoddeson:**



These are study sections where you went through Pauling's [Nature of the Chemical Bond] book for example?

**Bardeen:**

The interactions were closest among the people working in semiconductors, but we did have close interactions with all of the others.

**Hoddeson:**

Let's turn to the Shockley article (Shockley, "Path", op.cit.]. Now, he says that it was in April, 1945, on page 604, April 16, 1945 is the date of the notebook entry for the first two fields effect transistor designs.

**Bardeen:**

Hm mm.

**Hoddeson:**

Then apparently Brattain did the experiment in which, according to Shockley, he didn't see anything. According to Brattain, he didn't get a very large effect. He saw something but it was negligible. Shockley then said in his IEEE paper that on June 23rd, he estimated quantitatively the degree by

which the effects fell short, and brought his calculations to you, and then...

**Bardeen:**

This was in the fall...

**Hoddeson:**

He says it was June...

**Bardeen:**

...that may have been the time he did this, but I didn't join the group until, must have been October, I guess.

**Hoddeson:**

I see. It does seem strange that there should have been such a large time delay before you came back with an answer. He dates your answer as the 19th of March.

**Bardeen:**

He says he brought it to my attention, but that was not in June 1945. That was much later.

**Hoddeson:**

I see.

**Bardeen:**

I don't know what the date was. It couldn't have been at that time because I was still in Washington.

**Hoddeson:**

OK. And then it still took a few months before you came out with your theory. Or did perhaps your theory come out before you actually wrote it up?

**Bardeen:**

It was not too long after I heard about the experiments. I think that it was probably the result of a retrial of the experiments using silicon. Well, he tried it with several people. Using thin films of silicon.

**Hoddeson:**

..."had been deposited by Gordon Teal"... This is page 605...

**Bardeen:**

[reading] "showed there is a substantial"... so this must have been after he made these quantitative calculations because he talks about his calculations. But I know that Ohl deposited some films, measured by Pearson and others. This was one trial, but it was not the only one, by any means.

**Hoddeson:**

I see. So there was a series of attempts to measure the field effect.

**Bardeen:**

Particularly to make a quantitative estimate. We need to know the mobilities of the carriers in these thin films. And that requires a Hall effect measurement or something like that.

**Hoddeson:**

I see. It seemed to me at first when I looked at the literature that Shockley should have perhaps gotten the idea of surface states himself because he had written an earlier paper before the war on surface states for an ideal situation.

## Bardeen:

When he was at MIT as a graduate student, he got the idea of dangling bonds, thinking of surface states in terms of the chemistry picture of dangling bonds, and showed that you get dangling bonds if say you start with a  $sp^3$  P level, separated, as they are in the atomic state, if they cross over, so you find what you're doing is finding the tetrahedral bonds when the band cross over and the lower band corresponds to the electrons in the tetrahedral bonds from a chemical point of view. And what he showed is that if you have crossing bands of this sort, the surface states you also have primitive ideas of surface states where you assume some potential in the crystal and you have to assume where it's broken off, as it goes from the crystal to the vacuum and see if you have any bound states at the surface. He made calculations of that sort. This is one which made more sense, tied in with the chemical picture -- free bonds for electrons at the surface which could accommodate electrons --. But this, of course, you're talking about an ideal surface in this case, an ideal surface in a vacuum and such states associated with broken bonds have been observed. In actual surfaces you always have an oxide layer and with a surface

exposed to air, most of the dangling bonds will be filled up with something or other because they're chemically active.

**Hoddeson:**

When you told Shockley about your surface state concept, did he put that picture together with his earlier theory?

**Bardeen:**

Well, he accepted it right away.

**Hoddeson:**

He refers to I think in his article, as "one of the most active research topics in the transistor group."

**Bardeen:**

And as a result of it, we organized the experimental efforts so that Brattain concentrated his efforts on surface properties and attempts to measure surface state properties and things of that sort. While Pearson, who was the other experimentalist in the group, concentrated on bulk properties. Although it was an experiment of Pearson's which gave the first indication that the field effect really exists, because

you have surface states and you have a time constant associated with them, which presumably increases at lower temperatures. I suggested doing an experiment at low temperatures, which he did, and he observed an effect, but much smaller than we'd expected because of the low mobility of the carriers in the film, it was a deposited film -- it was probably crystalline and the crystals were extremely small, so the mobility of the carriers was very small, very low, was kind of on the borderline between amorphous and crystalline. And so you couldn't expect, a field effect based on bulk mobilities, because of the low mobility of the carriers in these thin films.

**Hoddeson:**

Would you say that Pearson's experiment at that time with low temperature was one of the crucial experiments to demonstrate the surface states?

**Bardeen:**

Well, it showed that there were two difficulties with the thin films. One was surface states and the other, the low mobility of the carriers. The idea suggested explanations for other things, already known, which hadn't been explained.

**Hoddeson:**

Yes, such as the fact that there is no contact potential between semiconductors of different conductivity type, for example.

**Bardeen:**

Or the fact that their rectification characteristics didn't depend on the nature of the point contact. You could even quite effectively make a point contact between two pieces of germanium and use that as rectifiers back to back so that you have to have a place, where charge could be placed at the interface.

**Hoddeson:**

I noticed in the on surface states that you discussed the work with Shockley, Herring and Brattain. I was wondering what contributions they made.

**Bardeen:**

Well, I don't remember specific ones now. There is often confusion as to what's in this, what's meant by surface states in this paper. Some people think I was talking about states on an ideal surface. Those are what Shockley was talking about, the theoretical



point of view. But I was using the concept more generally, as to any states that might exist at the interface, particularly the ????? of surface states, but also surface in fractions, foreign on the surface. On general grounds there's good reason to suppose that the ratio of the number of surface levels to surface atoms may be much bigger than the ratio of the number of impurity levels to atoms in the interior.

**Hoddeson:**

... page 719.

**Bardeen:**

To get enough surface states to effectively shield the interior, you need about 1 per 100 surface atoms order of magnitude. Now, in the interior, you might have one, the interior at that time, you might have one per million bulk atoms. You'd expect many more imperfections at the surface. So it's evident from this paragraph I was thinking of the concept much more generally than ideal sorts of surface states. Just any sort of states at the surface which could serve to trap electrons. One of the important points is, you don't need very many to shield the

interior, 1 to 100, so with the surface atoms the interior is effectively shielded.

### **Hoddeson:**

The next crucial step towards the point contact transistor seems to be a particular experiment that Brattain did, one of a series, in which he was measuring the temperature dependence of the contact potential at the surface of a sample of germanium or silicon. He noticed -- this is in Brattain's account -- he notice that condensation of water from the air on the surface caused a considerable hysteresis effect as the apparatus was brought from high to low temperatures and back.

### **Bardeen:**

And the humidity and things of that sort had large effects.

### **Hoddeson:**

Yes. The fact that the water was in some way affecting the experiment then led him to put the whole apparatus in water -- which looks like a somewhat drastic step. I wonder if you'd comment on it. First of all, was that a natural step to take,

simply to put the whole apparatus under water, in order to avoid hysteresis?

**Bardeen:**

Well, he was doing a lot of experiments which would give information about the surface barrier. And measuring the contact potential and the way the contact potential changes with light -- one of the most direct ways you can get information about the surface barrier. When you shine a light on it, you can see how the surface barrier is affected, how the contact potential is affected by the light. You can get information about the sign of the surface barrier.

**Hoddeson:**

But the accidental hysteresis seems to have led to a new area of exploration.

**Bardeen:**

He was doing a lot of experiments regarding the surface barrier, and I guess he thought dunking it in was another way you could get information.

**BAYM**

Did he realize that the fact that the water was affecting the experiment offered an important clue? Or was it just regarded as a general nuisance?

**Bardeen:**

Well, it indicated that humidity was having an effect, certainly; water condensing on the surface was having an effect on the experiment.

**BAYM**

In a sense that was exactly what you were looking for, such effects.

**Bardeen:**

We were looking for such effects, ways to control the surface barrier. Eventually we wanted to control it with an electric field, the field effect. That led to the experiments with Gibney in which Gibney, I think suggested using ethylene glycol, wasn't it?

**Hoddeson:**

Well, Brattain and Shockley say it was glycol borate. When I spoke with Gibney last January, he thought it had been monitol.

**Bardeen:**

We'll just have to go back to the notebooks to find out.

**Hoddeson:**

I think the notebooks say glycol borate. But, in any case, it was the condensation that led to the dunking of the whole thing in water.

**Bardeen:**

... it was the indication that water was having an effect on the surface.

**Hoddeson:**

And then Brattain noticed a much larger effect, when it was entirely immersed in water, a large change in the photo emp. And at that point, he showed the experiment to Gibney who apparently realized that the mobile ...

**Bardeen:**

...that the water was acting as an electrolyte, and putting in a real electrolyte, would lead to a larger effect.

**Hoddeson:**

What did you think was happening at that time?  
How did you think the water was affecting the experiment?

**Bardeen:**

Well, just I guess the water -- I didn't have too much of an idea whether it was a chemical effect at the surface, then when you put in a electrolyte and apply an electric field, yet got a big effect. It was evidently a large field produced by ions at the interface which overcomes the surface states.

**Hoddeson:**

There was no talk of anything like hole injection at that time?

**Bardeen:**

No, not at that stage. There were no indications we were doing anything except getting a large electric field at the surface. And they could measure the contact potential and change the contact potential of  
????

**BAYM**

Were holes being injected at that time?

**Bardeen:**

No.

**Hoddeson:**

This is November 17th, 1947.

**Bardeen:**

There was no rush of conduction between the electrolyte and the ??????? Just static effects. If you apply a field, you can get a slow electrolytic decomposition of the germanium. That's obviously not very large currents.

**Hoddeson:**

Did you have any idea of trying to make a contact at this point? This is November 17 we're still at.

**Bardeen:**

In this article of Shockley's ("Path", or. cit.] talks about Thursday, the 20th of November -- this is three days after they first observed this large effect of the electrolyte -- they wrote a disclosure proposing that this field effect with an electrolytes

could be used to build a field effect amplifier. But they didn't -- in the patent application, they showed as an example of how this could be done, the point contact on the surface -- this is one of the things which is not too clear in Shockley's article. The disclosure they made was the 20th of November, and it was on the 23rd of November Hoff tape)].

**Hoddeson:**

November 20th, 1947.

**Bardeen:**

This was a notebook entry which has three days after they first observed this large effect of an electrolyte, applying a field through an electrolyte on the surface barrier of -- let's see...

**Hoddeson:**

In connection with this, both Brattain and Shockley claim it was Gibney's suggestion to vary the potential bias. When I spoke with Gibney, he didn't remember whether that was his idea.



**Bardeen:**

I don't remember because I wasn't directly involved in it. But, in any case, they were both involved in it because they wrote this disclosure concept which eventually led to a patent. But originally this was, when they wrote this disclosure, it was just a kind of general idea that you could get by the surface states using an electrolyte and thus presumably, make a field effect transistor. But the idea of using a point contact to observe it on bulk material was mine, and it's in this entry on 23rd November.

**Hoddeson:**

Yes. Now, the entry on the 23rd of November was the entry that suggested the point contact arrangement in which the point contact is surrounded by wax and a drop of water.

**Bardeen:**

Yes. And the reason for doing that -- suggesting this arrangement -- it was originally just a very simple way of testing the idea to see whether this field effect idea would really work. Which they hadn't presumed that you could put on electrodes and carry

out the experiment. This was a simple way in which you could do it. And you'd get around two problems, with thin films, if you made use of an inversion layer on the surface. I'm talking here about a thin n-type layer on the surface of a block of p-type silicon. Instead of using an actual thin layer, about the only way we had to make them at that time was by deposited film which had poor electrical properties - - you could use bulk material with an inversion layer and thus you'd get the advantage of the high mobility in the bulk.

**Hoddeson:**

I see.

**Bardeen:**

And then the point contact geometry is just a very simple way of doing it.

**Hoddeson:**

I see. So using the inversion layer was simply a way of getting...

**Bardeen:**

...a way of getting effectively a very thin film but in bulk material so you'd have a high mobility...

**Hoddeson:**

I see. Why did you have to use such thin films?

**Bardeen:**

Oh, you have to use a thin film because with the sort of electric fields you can use, the number of carriers. The amount of change in resistivity of a film depends on the charge you can induce by the field, which is a fixed amount, and the relative change is greater the thinner the film.

**Hoddeson:**

I see.

**Bardeen:**

So you needed a very thin film to get a sizable effect. So this arrangement would get around the problem of the very low mobilities in deposited films.

**Hoddeson:**

Was this the first time that an inversion layer was used this way?

**Bardeen:**

This is the first time an inversion layer was used.

**Hoddeson:**

At all?

**Bardeen:**

That is in the concept of Brattain and Gibney, it was just a way you could vary the surface barrier up and down, but they hadn't given any specific arrangement in which this could be done.

**Hoddeson:**

I see. And then this actually worked, this apparatus with the point and the water drop. You observed amplification of power on the 21st of November. But I understand there were two problems, one being that the water drop tended to evaporate, and the second, that you only had low frequency response. Is that correct?

**Bardeen:**

Let's see.

**Hoddeson:**

I'm getting this all from Brattain's account.

**Bardeen:**

Well, I wrote it up on the 23rd which was a Sunday so I must have had the idea before that [laughter]. I didn't go into the laboratory on Sunday. It may have been on the 20th or 21st. It was the 17th when it was observed. It was the 20th, three days later -- it must have been earlier than that -- when they wrote their disclosure and it was the 23rd when I wrote my notebook entry.

**Hoddeson:**

Then there are a number of experiments that are described in Brattain's notebook and reprinted by Shockley on page 609, figure 13.

**Bardeen:**

Well, this is their figure 3, in the patent...

**Hoddeson:**

...figure 14 on page 609.

**Bardeen:**

Figure 14 is one they used in their patent to illustrate that you could use this effect to get a field effect transistor. But it was my idea of using the inversion layer in this geometry, although my name wasn't on the patent. They had the idea earlier that you could make a field effect transistor, but using an inversion layer with this geometry was my idea. They just used this as a kind of reduction to practice.

**Hoddeson:**

Now, two changes were made soon after. One to change the electrolyte, apparently to glycol borate, then called "gu".

**Bardeen:**

Yes, it didn't evaporate so rapidly.

**Hoddeson:**

And also to change to high back-voltage germanium, and that apparently was your suggestion. I'd like to know why you came up with that.

**Bardeen:**

Well, I don't remember the details, but according to Brattain's memory, that came as the result of a lunch discussion with Shockley. Brattain and Shockley and I apparently had lunch together.

**Hoddeson:**

Was Shockley very closely involved in these experiments?

**Bardeen:**

Not very. He was heavily involved after the discovery of the point contact transistor. But not so much before.

**Hoddeson:**

OK, well, then it changed to using....

**Bardeen:**

This is more just a kind of a simple experiment to see whether it would work, to see whether there was an inversion layer. We didn't know that there was an inversion layer in germanium. It's called high back-voltage germanium, it made good rectifiers. But...

**Hoddeson:**

Was it more pure than the silicon?

**Bardeen:**

It's essentially pure. That was the essential reason.

**Hoddeson:**

I see.

**Bardeen:**

It made good rectifying contacts, is the reason it's called high back-voltage germanium.

**Hoddeson:**

And you knew this from the work of the Purdue group?



**Bardeen:**

From the Purdue group, yes. They had the germanium available so the experiment was very easy to do. So, we thought we'd try it.

**Hoddeson:**

I see, but you weren't sure you'd get an inversion layer.

**Bardeen:**

And that worked much better than the silicon. It indicated that there was a p-type layer on the surface of the germanium. Then of course, we also knew from other experiments on germanium rectifiers that the barrier was present on the free surface, that is you have an inversion layer on the free surface.

Because if you made contact between two pieces of germanium, effectively a point contact, and he was doped very highly so it's essentially a metal, you got about the same rectification characteristics as you would get with a metal point contact. And from then on, most of the experiments were on germanium because the effects were larger on germanium.

**Hoddeson:**

I'm a little bit confused about this experiment. After you switched to germanium and to glycol borate, Brattain writes in one of his articles that you noticed a reversal of sign. And were surprised.

**Bardeen:**

We used P-type silicon with an N-type inversion layer, and in germanium, it was N-type germanium with a P-type inversion layer, so the signs were reversed.

**Hoddeson:**

You didn't realize that when you started?

**Bardeen:**

Oh yes we knew that it would if the experiment was going to work properly. Because we knew that it was N-type germanium, and rectified as N-type material, so if we were going to get an effect, it would be of the opposite sign. We knew we had an N-type inversion layer in the P-type silicon.

**Hoddeson:**

I see so it was no surprise.

**Bardeen:**

It wasn't any surprise. That's what we expected and that's what we found.

**Hoddeson:**

The surprise was perhaps that you could see, through the glycol borate that you were growing interference films in this experiment. Brattain mentions this.

**Bardeen:**

Well, the glycol borate was affecting the surface. It was gradually etching the surface, slowly etching the surface and making an oxide on the surface.

**Hoddeson:**

And how did that show up in the measurements? Did that cause the resistance to slowly rise?

**Bardeen:**

Well, it didn't show up in the measurements.

**Hoddeson:**

It was just a visible detection?

**Bardeen:**

It may have affected the interface, although the rectification characteristics are just about what you'd expect without the glycol borate, you just measure the reverse characteristics.

**Hoddeson:**

I mean the film. The interference films that were being grown under the glycol borate, you say that did not affect the results?

**Bardeen:**

That's probably an oxide.

**Hoddeson:**

Yes, the oxide that was being deposited.

**Bardeen:**

Well, grown.

**Hoddeson:**

Yes grown -- wasn't that insulating the electrolyte from the semiconductor?

**Bardeen:**

I don't think the oxide was that thick. It couldn't have been so thick you'd get interference from it. It was just a few layers of oxide.

**Hoddeson:**

Let's see, this is an interview that Brattain did in 1964 with Alan Holden. They discuss, let me find it, he say, this is Brattain, on page 30 of the Holden interview, that "In the process of working with these things, we found that if we put on some steady bias on the electrolyte, we got a bigger effect, put our AC on around some bias point, and then a steady bias on high back- voltage germanium -- it was in the anodic direction -- and we could see through the glycol borate that we were anodizing, growing visible interference film, green film. I can remember the green color under the glycol borate. We were unable to make the thing amplify much above 10 cycles. We reasoned that this was the slowness of the

response of the electrolyte, and...." OK, then he talks about the glycol borate being slow, and then he goes on, "So seeing this film we thought, ah, this oxide film must be insulating. If it is, we can form the film and put metal electrodes right on top of the film, get this field effect without the electrolyte, and get the higher frequencies."

**Bardeen:**

Well, that's what we were trying to do, is form a thicker oxide film on the surface. So it may be that you could grow a thick enough film, if you applied a steady voltage on it, but just doing the experiment to show that you got a field effect, I think we had effectively just a very thin oxide layer, not a thick oxide layer, not one comparable to the way it went with ??????

**Hoddeson:**

It seems that there was some confusion about how the oxide coat was functioning.

**Bardeen:**

We did want to -- this is described in my Nobel lecture, [J. Bardeen, "Semiconductor Research

Leading to the Point Contact Transistor," Les Prix Nobel En 1956 (Stockholm 1957).], which exactly quotes these experiments between the 8th and 16th of December.

**Hoddeson:**

Yes, this is on page 611, the first column.

**Bardeen:**

Although it worked well as an amplifier, it could amplify only very low frequencies and we knew that if you have the sort of ????? parts you have in the electrolyte alkali, you're never going to get up to high frequencies. And so this is not a practical way of making an amplifier. So we wanted to get rid of the electrolyte, and at the same time, to get a higher electric field, so we wanted to get a very thin oxide layer.

**Hoddeson:**

Now, were you in effect trying to have the oxide layer play the role in the experiment that the electrolyte played previously?

**Bardeen:**

Yes, so you wouldn't have an electrolyte, you'd have just an oxide layer, we'd evaporate metal over the oxide, and hopefully the oxide would be insulating. But the oxide wasn't to that extent.

**Hoddeson:**

[Laughter] Before we get to that step, I just want to ask, did you think that the oxide layer would be polarized in the experiment? How would the electrical effect be communicated through the oxide layer?

**Bardeen:**

It would just act as a condenser.

**Hoddeson:**

Yes.

**Bardeen:**

An MOS transistor, metal oxide semiconductor. The oxide was very thin. You can get a high electric field when you apply just a small voltage across a small distance. To go back to the original idea of getting a



field effect, with this geometry we knew that we had an inversion layer and we got around the mobility problem of a thin film. So you could get a high electric field, with a moderate voltage, then you could make an amplifier.

**Hoddeson:**

Gibney remembers that after you noticed that you were accidentally growing an oxide layer, he had the idea of intentionally anodizing.

**Bardeen:**

Creating a thick oxide on which we could evaporate the gold. See, the transistor here (pointing to a figure in Shockley "Path" article).

**Hoddeson:**

This is page 611 and it's the original.

**Bardeen:**

Figure 16, the original point contact transistor, you can still see the gold spot on that piece of germanium although we weren't using it in this experiment.

**Hoddeson:**

I see.

**Bardeen:**

This little gold spot right here. But before that, Walter Brattain observed a small amplification, voltage amplification or power amplification, by putting the point contact right near the gold spot, and using the gold spot as the emitter and the point contact as the collector.

**Hoddeson:**

Now, this I think is the crucial step.

**Bardeen:**

That's the one where first, and this was in the opposite direction, whereas in the field effect, in this case, where you have an N-type germanium with a P-type surface layer, if you apply a positive voltage, you tend to drive out the holes if it's a field effect and decrease the current, that's what we observed. But in this experiment with the gold spot, we observed when we applied a positive voltage, an increase in current, not a decrease.

**Hoddeson:**

Then you know you were contacting.

**Bardeen:**

Then we knew that we were not only contacting, but somehow introducing carriers into the layer.

**Hoddeson:**

And this is the first time you've talked about hole injection?

**Bardeen:**

Yes. It's the first time.

**Hoddeson:**

Now, in Brattain's account, it appears that this contact was an accident. He claims he accidentally washed the oxide layer off when he washed off the glycol borate and that caused the contact. Is that the way you remember it?

**Bardeen:**

Well, as I remember for whatever reason, the gold spot wasn't insulating in the way we'd hoped. We

though we'd try and see what happened anyway.  
[Laughter].

**Hoddeson:**

There's a discrepancy between Brattain's account and the official Bell Laboratory account put together by Gorton. [W.S. Gorton, "The Genesis of the Transistor", Case 38139, December 27, 1949, 1100-WSC-XB ]. Gorton says, on page 5, beginning of the first full paragraph, "In order to increase the frequency response, Bardeen suggested replacing the electrolyte by a metal contact--

**Bardeen:**

-- and an oxide layer.

**Hoddeson:**

But Brattain, again in his interview and also in his Physics Teacher article W. Brattain, "Genesis of the Transistor"], claims that, let's see --

**Bardeen:**

Well, there's really no discrepancy. Just that this is not a good way of forming --

## **Hoddeson:**

Let's see, Brattain said, this is the bottom of page 30 of the Holder interview, "I inadvertently shorted the point of the gold film in the nice center hole that we had fixed up very early in the experiment. So I got practically no data there was disgusted with myself, of course, but decided that there was no reason why I shouldn't go around with the point, around the edge of the gold, to see if there was any effect, even if the gold was covering half the surface. I got an effect of the opposite sign. I got some modulation," etc., and then he realized that he had washed the oxide film off. It sounds like an unexpected effect, a true accident to make the contact. But this account by Gorton's makes it sound as though it was a designed step.

## **Bardeen:**

Well, the anodizing and the evaporated gold contact were supposed to design it to make an insulating oxide layer between the metal contact and the germanium surface. But the gold spots were not insulated, so that, of course, you couldn't tell when you just measure that, the resistance across, to see

whether they're insulating or not. Whether you must have a few spots where the film was gone or whether it was essentially making contact almost everywhere. And I think the latter was true, as Walter says in his statement. But we thought we'd see what happened anyway, even though it wasn't insulating. Then we observed this small effect.

**Hoddeson:**

But there was no power amplification?

**Bardeen:**

No power amplification. There was some voltage amplification because it indicated holes were going into the contact, the gold was supplying holes to the surface layer because the current was enhanced and the reverse current was mostly holes going into the contact rather than electrons going out. And you were enhancing that current so you had to be introducing holes.

**Hoddeson:**

Was this clear at the time? Did you understand it as well then as you just explained it to me now?

**Bardeen:**

That's the way I understood it. But what wasn't so clear was just how these holes were flowing in the surface. I'm not sure -- some of the later discussion in Shockley's paper takes up the point about hole injection. We knew we were introducing holes. Recently, came the designation of calling one of the contacts the emitter and the other the collector. Originally, they called the base electrode, the control electrode but then this didn't look very satisfactory because if you're going to see subscript, you have a C for the Collector and C for the Control electrode. And so they started using the base as the control.

**Hoddeson:**

Ok, we're on again.

**Bardeen:**

Then, right after this came the suggestion for the first point contact transistor to make the two contacts very close together. We originally thought that we would get a better effect if we had line contacts, that the reason...

## **Hoddeson:**

I see, these were actually line contacts.

## **Bardeen:**

Line contacts, yes, that's the reason for this geometry, to get two line contacts very close together. But then it soon became evident that you really didn't need lines if you had enough current to the collector. The field of the collector would draw in the holes so that the point worked just as well. Walter got the idea of grinding off a plain case of a point contact, so you could put two point contacts very close together. This geometry with the quartz wedge and gold on the side wasn't used very long. It served for the first experiments and worked when we tried it, first time we tried it using the same surface which had been anodized with the gold spot on them. And with this line contact, we didn't have extremely high resistance in the reverse direction. With this gold contact, the resistance in the reverse direction wasn't as high as it is on a normal high back-voltage rectifier where it's extremely high so you don't draw very much current. And then, one tried to use point contacts, they worked well if their



reverse resistance wasn't too high but in many cases, the reverse resistance was too high and so it didn't work. It depends on the nature of the surface and what contact you put on. What we thought was happening was that if you had a good inversion layer on the surface, then the resistance would be low, but if the inversion layer wasn't all that great, then the resistance would be higher. Later we found out that using tungsten contacts, I think they were tungsten-bronze, if you passed a large current through a collector, that you so-called "form" the point contact and then it does have more resistance, and then you could do this every time. It isn't so critical as to the nature of the surface and the contacts. You could form, what you call form, the collector. Evidently, what you were doing was forming a small P-type region when you formed the collector. It also reduced the resistance from what you get with a normal point contact.

**Hoddeson:**

I see.

**BAYM:**

Did you understand what was actually happening in the forming process, how the physical properties of the surface were actually being modified?

**Bardeen:**

It melted a little region around the surface which introduces impurities.

**Hoddeson:**

Did you understand that at the time?

**Bardeen:**

We had a pretty good idea of what was going on.

**Hoddeson:**

Were you aware of Bray's work at Purdue on the "spreading resistance" at the time you first began to see hole injection?

**Bardeen:**

I don't think we tied it in immediately. But in the sequence of events which followed, December 23rd when we were trying to get the patent applications in as rapidly as possible, because we didn't want to get scooped on the discovery, and wanted to get the

patent applications in so we could eventually publish. I was assigned the job of working with the patent attorney so a large part of my time for the next month or, next couple of months, was spent with the patent attorney who was one who hadn't known anything about semiconductors. I had to start from scratch to teach him something about semiconductors.

**Hoddeson:**

This was Hart?

**Bardeen:**

Yes, this was Hart. And I tried to get the applications written. So, a large part of my time was taken up by that. In the meantime, Shockley jumped in with both feet. That's the time he really got excited and spent all his time on it. And he talks here about it in his article, let's see if I can find it. Well, our thinking up to that time....

**Hoddeson:**

... this is page 616 [Shockley, "Path", op. cit.].

**Bardeen:**

This is from December 23rd until, let's see if I can find the date here -- it's the date when John Shive gave a talk to the group, showing that you could make a point contact transistor with points on the opposite sides of a thin wedge, which showed that the holes must be going in from above. That was, I think, early February.

**Hoddeson:**

Oh -- was it that late?

**Bardeen:**

Let's see if I can find this. Well, I think it was early February, a little over a month later.

**Hoddeson:**

Oh, yes, page 618, he says "Striking experimental evidence for injection...."

**Bardeen:**

...it's reported, 18 February by Shive. And when Shive was talking, I immediately came to the conclusion that that's what was happening and then

Shockley says that, following that, he gave his -- his first announcement to the group of a junction transistor structure which he thought of as a way of trying to understand how the point contact work, and this involved the injection in the bulk rather than depending on the surface layer. And this is in late January. It was about a month later that he came up with the junction transistor structure, which would be about the 20th of January. And then this was the 18th of February, about two or three weeks later, when independently Shive decided to see whether he could make a transistor to work with two point contacts, the opposite sign, which showed definitely that holes are being injected into the bulk.

**Hoddeson:**

Was this the first definite demonstration, that what you were seeing was holes injection?

**Bardeen:**

That was the first definite evidence. And then Shockley earlier got this juncture transistor structure for which he made calculations.

**Hoddeson:**

You pointed out in several places, for example in your Nobel lecture, that the Bell program differed from programs elsewhere in understanding the significance of the minority carriers, the holes. Why hadn't others realized this? Was it such a far out concept at that time?

**Bardeen:**

Dealing with non-equilibrium phenomena. I think people are just not used to dealing with non-equilibrium phenomena of that sort. And our thinking was biased toward the inversion layer on the surface, because that's the way the experiment started and clearly demonstrated that it was there and we were trying to understand why some point contacts would work and others wouldn't; some would have high resistance and others low. It seemed to suggest that it was the inversion layer on the surface and also it seemed to depend on the way the surface was treated. The one we used here was anodized, and that seemed to be a way of getting the low resistance in the reverse direction, in other words, a large inversion layer on the surface. And so

it was unclear. Holes were obviously going in to the bulk and in the inversion layer because if they're in the bulk, they're even more in the inversion layer. If you increase the concentration in the bulk, you increase them even more in the inversion layer. What you're doing is essentially raising the Fermi level for holes, increasing the level for holes. If you increase them in one place, you increase them in the other. It was not clear at the beginning which effect predominated. And we were thinking more in terms of the surface. But his experiment of John Shive showed that the surface layer was not essential at all, they could go through the bulk. It was a very clear demonstration of that. But I think that that whole program I think perhaps got distorted with the use of the point contacts which was originally introduced just as an easy way of doing the experiment, just to show the effect. A little later we made a transistor and then the emphasis was on trying to improve the point contact transistors, rather than going back to the beginning, and trying to make better devices which we only did much later. Of course, the whole technology had to develop where you diffuse in impurities of appropriate amounts and the growth of single crystals by Gordon Teal was an important

growth of single step. The first good junction transistor was the grown junction, in which junctions are introduced as you grow the crystal from the melt. These had good characteristics, but were expensive to make. I think that if the orientation of the program had been otherwise, say following up, more of what was going on in forming and how to do things on the surface, we might have come to surface barrier transistors and integrated circuits and things like that a lot quicker than actually happened.

**Hoddeson:**

I would like to ask a question on the earlier experiment in which you were using the deposited gold spots. If the shorting out hadn't taken place, would that experiment have worked the way it was set up?

**Bardeen:**

Oh, it worked, yes, like the present MOS transistors.

**BAYM:**

So either way, you would have observed transistor action.



**Bardeen:**

Yes, mm [laughter].

**BAYM:**

Wow [more laughter].

**Bardeen:**

In the patent application in which I probably talked about the point contacts too much, we probably could have gotten broader claims. That was essentially this same geometry, but using an insulator layer with a metal deposited over it to get the field effect. That's essentially what the MOS transistor is. Although, as Shockley points out here, eventually, when we didn't have an inversion layer at the start, by applying a large enough voltage, you could swing the surface over, to make an inversion layer, even though it wasn't present initially. And that didn't come along until quite a few years later.

**Hoddeson:**

What did you say the first time the circuit was actually spoken over, on December 23rd? Do you remember?

**Bardeen:**

Huh?

**Hoddeson:**

Brattain says on December 24th, in his notebook, that the circuit was actually spoken over on the 23rd. The notebook entry is on the 24th. He says on the 23rd, it was actually spoken over. "The power gain was of the order of 18 or greater." My question is, what did you say when you spoke over the circuit?

**Bardeen:**

Well, I said quite a few things. I've forgotten what they were. I think Shockley says it's too bad we didn't use the words which Bell used, "Watson, or something like that, are you there?" I wouldn't be surprised if we didn't actually use those words.

**Hoddeson:**

It must have been a very heady occasion. Before we close, I wonder if you could comment just a bit about the seven month period between the discovery and the public announcement. This is the period when you were involved with Hart.

**Bardeen:**

I was involved with Hart a good bit of that time.

**Hoddeson:**

It must have been very difficult to keep it quiet.

**Bardeen:**

Yes, it was "laboratory confidential", so it couldn't be mentioned outside the immediate group and also, patent attorneys were trying to get -- the original case was re filed, not so much in the disclosure, but to get more comprehensive claims. I think the original one had half a dozen claims. I think there were over 50 in the final version. And I learned a lot about patent law. Hart learned a lot about semiconductors during that time and then in the meantime, Shockley was also getting his patent on the junction transistor.

**Hoddeson:**

Was Hart working with Shockley also?

**Bardeen:**

No, another patent attorney was working with Shockley.

**Hoddeson:**

In this period, Brattain recalls going to a APS meeting in January and hearing a paper by either Benzer or Brey in New York on the spreading resistance, and having to say nothing because of the secrecy. Were you at that meeting also?

**Bardeen:**

I expect so, I don't remember. I remember the paper on the spreading resistance.

**Hoddeson:**

Do you remember who gave it? There were two papers given actually, one by Brey and another by Benzer. I think was the Brey paper.

**BAYM:**

One of those was in Washington.

**Hoddeson:**

Oh, that's right, the Bray paper was in April in Washington. You're absolutely right. So it must have been the Benzer paper in January.

**Bardeen:**

The resistance in the forward direction was ten times lower than was accounted for by spreading resistance.

**Hoddeson:**

And you couldn't explain it to him.

**Bardeen:**

As in many cases, when you find an explanation for a new effect, or an explanation for one thing, you can explain other things with it too. Now we were very concerned, particularly with those papers, that they would come up with a discovery of the transistor before we did.

**BAYM:**

Did you go to Washington?

**Bardeen:**

I think so. I don't remember it now.

**Hoddeson:**

Did the Purdue group suspect that ..

**Bardeen:**

... I don't think so. I don't think they had any idea. It came as a complete surprise to them. After the announcement, a lot of people were able to repeat the experiment, within a day or so. Once you have the idea, there's no problem repeating

**Hoddeson:**

Were there any hard feelings between the Purdue and the Bell people over the transistor?

**Bardeen:**

I don't think so. If they'd had the idea, there might have been. But they didn't so there wasn't any hard feelings. We tried to give them credit for the work they did which was very fundamental to ours of course, on germanium during the war and after the war.

**Hoddeson:**

Do you remember any specific reactions to the discovery at Bell, from Kelly, or Bown or anybody else? After all, this was the fruit of all the planning that Kelly had been making for the past ten years, on getting a solid state program at Bell. And it finally paid off now. He must have been...

**Bardeen:**

Of course, they brought Jack Morton in to head up a device development group and expanded the research. Work expanded quite rapidly. One aspect we haven't discussed was the work of Gordon Teal on single crystals, which was very essential to the subsequent development. Gordon Teal has written a good bit about Shockley was not going to back him in making single crystals because he thought he could make single crystals or cut out small single crystals from the poly crystalline material which were enough for research purposes. But Jack Morton eventually gave him backing but he had to do it essentially on his own, bootlegging it.

**Hoddeson:**

OK, well, thank you very very much. This was an extremely good session.



## Interview Session – 6

### **Hoddeson:**

The purpose of this discussion is to answer a few questions that occur to me on re-reading the first draft of my article on the transistor. Sorry to have bothered you with it. OK, first of all, I wonder if you could just go over once more the difference between what Shockley meant by holes, and what you and Brattain, when you first realized that holes were being, were entering —

### **Bardeen:**

There wasn't any difference in what we meant by holes. The only difference is how the holes flowed from the emitter to the collector. In our earlier experiments, the surface barrier of [semi] conductivity type, the inversion layer, played a very important role. It plays a role in the field of practical experiments, and when the transistor in fact was first observed, the influence on the collector current of opposite sides to that expected for the field effect, we knew we were observing something new, and the obvious explanation is that through the metal contact at the emitter, the emitter contact, holes were being

introduced into the semi-conductor, and flowing to the collector.

**Hoddeson:**

Yes. And Brattain had a note in his notebook, using the words that he realized that it was holes.

**Bardeen:**

Yes. The words emitter and collector were introduced very early, also indicate that we had the idea that holes were being emitted at the emitter and flowing to the collector. Originally it was not called a base contact. We called it a control contact, as it plays a role somewhat analogous to a grid in a vacuum tube. But it wasn't too satisfactory, because control and collector both start with the same letter, so if you use subscripts to designate them, if you designate them by the first letter, you have a problem. So we decided to call what we first called a control electrode, the base electrode. So I don't think there's any, as far as this part I've discussed, there is no problem. The difference between ourselves and Shockley came in the picture of how the holes flow from the emitter to the collector. They could flow predominantly through the inversion layer at the

surface, which does contain holes. And the collector would be draining out the holes from the inversion layer. They could also flow through the bulk of the — another possibility, that they could flow through the bulk of the semi-conductor, with their charge compensated by the increased number of electrons in the bulk. Shockley's idea for the junction transistor was based on the fact that holes introduced by the emitter in the junction transistor flow by diffusion to the collector, which is very close by the emitter, rather than through the base electrode, or what was earlier called the control electrode. In order to distinguish this from the flow in the surface barrier region, he introduced the concept of injection. Or he introduced the word injection. I wouldn't say concept so much, but he, to distinguish the hole flow through the bulk from hole flow through the surface layer, he used the term injection to designate hole flow through the bulk.

### **Hoddeson:**

In Shockley's long article on the discovery of the transistor, he writes that, "when the transistor effect was observed, in the sense of the birth of the point contact transistor, no one had any idea that minority

carrier injection, injection was being observed, any more than had been true for Bray's results. That is I guess just wrong? He goes on, "So far as I know, the first suggestion that minority carrier injection might be an important mechanism was made five weeks later in my disclosure, 23rd of January." I guess he just had it confused?

### **Bardeen:**

Larry is talking about flow through the bulk. Brattain was making some experiments described in the PHYSICAL REVIEW LETTER published with the Letter on the Transistor — of the way holes flow from an emitter contact, by measuring the potential as a function of distance from the emitter, to see whether the holes are flowing permanently, predominantly through the surface layer, which would be two dimensional flow, or through the bulk, which would be three dimensional flow. If you have an inversion layer at the surface, as we certainly had, in these early experiments, you increase the Fermi level for holes in the vicinity of the surface, and the Fermi level is the same as, in the bulk as in the surface layer, you're going to get, introduce, increase the concentration of holes more in the

surface than in the bulk. So it was not clear until more experiments were done just how the holes were flowing. In fact I think that they could flow - from what we know now, they could flow either way. The primitive experiment which showed they were flowing through the bulk was by John at the meeting you describe in your account there. He placed two contacts on opposite sides of a very thin wedge and showed that it worked just like a point contact transistor, with two contacts on the same side of a slab, which showed definitely that the holes were flowing through the bulk of the germanium, and that you could make a transistor this way. His transistor had characteristics which were very similar to those of two contacts on the same side. So there couldn't be any essential difference in the way they operated.

**Hoddeson:**

Now, the Purdue group had been —

**Bardeen:**

— in our patent disclosure, we mentioned both mechanisms. We said that the transistor could work either way.

**Hoddeson:**

The Purdue group had observed the spring resistance, in the high vac voltage germanium . Was there any understanding at that time that this was due to hole injection?

**Bardeen:**

I think when we first learned about the Purdue experiments, it became obvious. The explanation became obvious that conductivity was being increased in the vicinity of the emitter contact, the point contact, by emission of holes, which increased the number of carriers. And this made us somewhat concerned that maybe the Purdue group would also think of the transistor, and so, anxious to publish as rapidly as possible, to get priority on the discovery.

**Hoddeson:**

Did you feel that you knew all of the work that they were doing at Purdue? Was there any lack of information? on their side?

**Bardeen:**

Not as far as I know. More than is normal, when you do research. You generally wait until you're fairly well convinced that what you have is a complete and reasonably convincing story, before you publish it or talk about it at meetings. I don't think the way, they handled it was any different than any other scientific discovery involving publishable research. During that period, between the discovery and the first publication, just about six months later, we were of course confined with what we could talk about to other people, outside this small group in the company who were in on the discovery.

**Hoddeson:**

I'm interested in how you learned about the details of the work at Purdue and the other laboratories. Was it through publications or written reports or visits?

**Bardeen:**

Well, first information, when we first started working, is, we had reports from the Radiation Lab at MIT and also from Purdue Pennsylvania, which

were progress reports of work done during the war. And we read those. Later, the work of Tory and Whitmore on crystal rectifiers came out. I think it was published in 1948, which is giving the complete account of the work done in that Radiation Lab, somewhat also discussed the work done at the other laboratories, although not in as much detail. We depended at first primarily on these reports, and then later on publications, as after the war they began to publish their work. We depended on publications or discussions at meetings, papers presented at meetings.

**Hoddeson:**

Old you all read all of the reports, or did you divide up the material amongst you and report on it?

**Bardeen:**

We didn't divide up the material. I think people just read as much as they wanted, until they thought they had a reasonably complete picture, as much as they wanted, of what work was done. Some of the mathematical work was, I'll say that by Vivian Johnson, was done in considerable detail. I don't know that anybody plowed through it all. But we got



a pretty good picture in general of what they were doing.

**Hoddeson:**

[John Bardeen on the period prior to the transistor being made public and the fear that the military might classify it.](#)

Incidentally, you were talking about the secrecy period. Just before the transistor was made public, let's see, I think first the military were informed.

**Bardeen:**

Yes, they were informed.

**Hoddeson:**

And then?

**Bardeen:**

— and we hoped they wouldn't classify it, so we couldn't publish. But we had already sent in manuscripts to the PHYSICAL REVIEW for publication. They were told to hold them back until we got word from the military that they wouldn't be classified.

**Hoddeson:**

Why was there some concern that the military would classify them, the information?

**Bardeen:**

It wasn't done under any military contract or anything like that.

**Hoddeson:**

Was there any support from the military?

**Bardeen:**

There wasn't any support from the military. It was entirely supported by Bell Labs. But any important discovery which might have military applications, it was desirable to clear it with the military.

**Hoddeson:**

I see, just a preventive —

**Bardeen:**

— just as a courtesy.

**Hoddeson:**

I see. OK.

**Bardeen:**

I don't think the military recognized the importance of it. Perhaps not. Besides, it was so broad that, the possible applications would be so broad that it shouldn't be classified.

**Hoddeson:**

OK.

**Bardeen:**

I believe that maybe in your story, that people who went down to talk with the military were told that similar work was being done in one of the government laboratories. It turned out not to be the case.

**Hoddeson:**

I don't know that story at all. Where can I learn more about that? I don't know about that.

**Bardeen:**

You don't have anything on that?

**Hoddeson:**

I don't think so. No. Which government laboratory?

**Bardeen:**

I think it was the Naval Research Lab. I think it may have been in some of Brattain's accounts. I think he was one of those who — went down —

**Hoddeson:**

I'll have to pursue that. OK, now I'm going to skip around a little bit. I have some —

**Bardeen:**

OK.

**Hoddeson:**

— questions from several years earlier. Well, maybe they're not that— this particular one has to do with the relevance of All's ? work. Now, All was the man who picked up on Southwick's return to the tee of cat's whiskers, and —

**Bardeen:**

Not just a return — point contact diodes.

**Hoddeson:**

Right.

**Bardeen:**

As detectors for radar.

**Hoddeson:**

Right. And he experimented with many materials, and discovered that silicon was the best one to use. And then, in working with the metallurgists to obtain pure silicon, he — Jack Scaff and Henry Tonura prepared an ingot with junction — which Awl then discovered.

**Bardeen:**

Yes. He tried to make them more uniformly, with more uniform characteristics, with the old Lena crystal, you had to hunt around for a so called hot spot, you see. To find a sensitive spot where the rectification would occur. Just not very satisfactory for a production item.

**Hoddeson:**

No.

**Bardeen:**

And I think the use of silicon was mainly to try to get better uniformity in the product.

**Hoddeson:**

Now, how did, how would you evaluate Old's contributions with — in regard to the development of the transistor? And how, could -

**Bardeen:**

It's background for work done at the Radiation Lab, on the development of point contact diodes. And the reason we used point contacts in our experiments is, really just because it was easier to do, not because it was the ideal geometry or anything of that sort, but just that we weren't interested in going up to very high frequencies, so we didn't have to use a — point contacts for that reason, and so, we just used point contacts because there had been some art about using them and we knew what metals to use, and how to contact the semiconductor. And how to make

a rectifying contact. So both the first field effect experiment using point contacts and dropped electrode and the first transistor experiments which followed from that we used the point contacts, just for convenience, and the geometry chosen was just for convenience, just because it was something easy to do. The sort of experiment you can set up and do in one day. And, there was no other reason for it than that.

**Hoddeson:**

Well, you were the one who suggested that geometry.

**Bardeen:**

Yes.

**Hoddeson:**

After the —

**Bardeen:**

— one reason for suggesting it, is the earlier— for the field effect — whereas the other field effect work which by and large failed, was not only because of surface states, which we, could be gotten

around by going down to very low temperatures, but because the mobility of the carriers in the thin vapour layers which were being used was very poor. In other words, the electrical character properties of the thin deposited film, on which the first field experiments were done, was very poor. And so I got the idea of using an inversion layer, to get the effect of a thin layer by using the inversion layer on the surface of bulk germanium, and we hoped to — that you'd have good bulk properties but still have the effect of a very thin layer. And we took that first work with silicon because Brattain had contact with All and knew that with proper treatment you could cause an inversion layer on the surface of silicon. The experiments were first done, the first field effect experiments were done with silicon. When we did the experiments with germanium, we didn't know there was an inversion layer there, but that was one of the — it was easy to do the experiment and find out from the experiment that there was an inversion layer there. And it turned out that there was. There was some suggestion of it before, because of the very good rectification characteristics, we knew that there was a high barrier, perhaps sufficient to make an inversion layer so we had hopes that it would



work, and it did work. I said earlier that, in the field effect experiments, the inversion layer was very important, in that what you were modulating was the conductance of minority carriers in the inversion layer. And we probably had that too much in our minds, in interpreting the results, the first results of the one contact transistor, in that we knew that there was an inversion layer there which would provide a channel for holes going from the emitter to the collector. The question is, how important that aspect was, compared with the flow of the holes through the bulk, which Geoffrey [??] calls injection, to differentiate it from the word emitter which we'd already introduced. We wanted a different word to describe the flow in the bulk. And in our case, the flow was by the electric field of the conductor, and in his junction transistor, it was called diffusion through bulk, so there's a difference in the way the flow takes place. In Shives' experiment, he showed the holes were flowing through the bulk again? The electric field of the collector current no doubt played a role there too. So it wasn't purely diffusion.

**Hoddeson:**

Was the discovery of the PN junction important, essential to the work on the transistor?

**Bardeen:**

I guess it depends on what you mean by PN junction. The inversion layer at the surface is really a PN junction. And an inversion from N type say in the bulk to P type in the surface, in a sense, that's PN junction. In that way it played an important role.

**Hoddeson:**

Would you have hit on that all by yourself without it having been previously discovered?

**Bardeen:**

It doesn't require any doping of the — it's dependent on the inversion layer and the surface properties there, rather than introducing impurities into the bulk, so it didn't make a — if you think of a PN junction as due to donors and acceptors, in the bulk, in the first transistor we didn't have that. We just had inversion layer due to the surface properties.

**Hoddeson:**

So in fact, although I don't want .... (off tape)

**Hoddeson:**

...but I'm just trying to figure out whether or not the first transistor could have been developed without the previous work, though probably the work on silicon wouldn't have gone on as rapidly, if the earlier work on silicon hadn't been initiated.

**Bardeen:**

The fact that we had people in metallurgy able to make fairly pure silicon — it was also known how to dope it, either — I think that fact that we had metallurgists available was certainly very essential to our program. And that also supplies us with germanium, again, bulk polyfissionable material.

**Hoddeson:**

To what extent were you looking for a semiconductor amplifier in the period 1945, '46, from the time you came to Bell Labs, up until that first crucial experiment which Brattain did which made him realize that the electrolyte was having an

effect, which took place on November, 1947? In the period — did anybody ask you to look in that direction, either Kelly or Shockley or?

**Bardeen:**

I think it was in the back of everybody's minds, that that was one of the goals of the program, and could be attained, and Shockley was pushing the field effect, and showed that at least theoretically, it should be possible to make it.

**Hoddeson:**

Then he brought his calculations to you, and you figured out that the calculation was OK, but he had left out the effect of surface states. Right?

**Bardeen:**

That was later. First, he — at first I just checked his calculations, and they looked OK, but should be observing the field effect, but the experiments which were carried out — we weren't able to see anything. We tried a number of times, a number of different people, and that was some time later, I don't know just how long, after these reported failures, that — I'm sure it was done after I first checked his

calculations - that I came up with the idea of surface states.

**Hoddeson:**

Do you remember how you came upon the idea?

**Bardeen:**

Some time in 1946.

**Hoddeson:**

Let's see, your paper came out in March, 1946. On the surface state.

**Bardeen:**

'46 was it?

**Hoddeson:**

Yes, March. Shockley says that sometime in the winter or fall, the previous fall or winter, he had brought his calculations to you.

**Bardeen:**

I think that's fair.

**Hoddeson:**

So you were thinking about it for quite some time at least three, four —

**Bardeen:**

— well, a few months.

**Hoddeson:**

Yes.

**Bardeen:**

It wasn't anything immediate, because I think it was hoped initially that we would find a scheme which would work, but .... was unable to.... The silicon or germanium that they were using, deposited films, these materials — I suggested cooling them down so the surface states wouldn't come into equilibrium, and trying the experiment — with small effect, but smaller than you'd expect from the bulk mobility. This indicated that the mobility of the carriers was far smaller in these deposited films than effect it was in the bulk, and so that the effect you might expect to observe even in the absence of the surface states

was much smaller than the initial calculations, indicated.

**Hoddeson:**

By initial calculations, you're referring to calculations done using the mop shop key theory?

**Bardeen:**

Initial calculations were made using the mobility which is known for bulk material, and introducing thin film, so that we had a thin film bulk material with the properties of bulk material, no surface states, you should have gotten a reasonable field effect, which wasn't observed. And the two reasons for it were, one, the surface states, and, the other, poor mobility. And so, when Brattain and Gibney came up, showed that you could modulate the surface barrier with an electrolyte, I thought of this point contact geometry as a way of trying to observe a field effect on bulk material. I think that that was an important step in going from thin films to the bulk material with the good electrical properties. And then, we used the inversion layer, to get the effect of a thin film, and it was essentially the principle of modern MOS transistors.

**Hoddeson:**

Now, there was a period of one year and eight months between your paper on the surface states, and the experiment by Brattain to show the modulation....?

**Bardeen:**

All these experiments were done during that period.

**Hoddeson:**

In that period was — I mean, what were the group's expectations regarding the possibility of building a semiconductor?

**Bardeen:**

I was trying Debye's [??] experiments, to determine the concentration of surface states. When the surface states paper came out, the program was divided into, one mainly on surface properties under Brattain, and another mainly on bulk properties under Pearson. And the, Brattain's experiments and — were designed to learn more about surface states and surface barriers, and there were quite a number of experiments carried out which confirmed the idea



of surface states and gave some estimate of the concentration.

**Hoddeson:**

But did you feel that if you understood the phenomena well enough, the surface states and whatever else you would learn about the semiconductors, that eventually you would as a group discover how to build —?

**Bardeen:**

— yes, then the field effect should work. And the amplifier.

**Hoddeson:**

I see. So you never really lost hope that the thing should —

**Bardeen:**

— oh no —

**Hoddeson:**

— throughout this entire period. Because it depends writing on who's writing the story. Brattain, when he writes his accounts, tends to stress the basic

research aspect of the work, and of course it was basic research —

**Bardeen:**

— it was basic research, because we were trying to understand surface properties, and we were not trying to make, by any empirical ways, new transistors or anything like that. We were trying to learn about nature and the semiconductor surface.

**Hoddeson:**

But the purpose of this research — supported by —

**Bardeen:**

— and then, when we were able to bring the surface states under control, we would be able to make a — so that — it was a program of basic research.

**Hoddeson:**

Yes, but it still had the ultimate goal —

**Bardeen:**

It had the long range goal to understand this and bring it under control. Then we would make a major advance. I think that's sort of basic research which

should be done in industry, is to understand more about the products, so it's not just empirical but real understanding; so if something goes wrong, you know what to do to fix it, or if an important advance can be made, make that too. So you will understand what the limitations are, and what is possible so you know how far present projects are from what's theoretically possible.

**Hoddeson:**

What were the most important experiments, as you recall, that were done in that period, between the surface state paper, and the start of the experiments on modulation which went, you know, which led you back to the field effect?

**Bardeen:**

Brattain did a series of experiments on measuring contact potential and change in contact potential with light shining on the surface, which gives information about the surface barrier. And this was — and then in addition, to experiments of this sort which were very important, experiments were done to try to see whether surface barrier was already present on germanium when you put a metal in

contact with it, and this seemed to be the case, because the rectification didn't depend much on the metal used for the contact, and you could also use for the contact heavily doped piece of germanium which had high conductivity, when, if you made a contact by having two wedges perpendicular to each other, with the edges of the wedges touching one another, that they just touched essentially at one point, so it was like a point contact. And this had characteristics of, similar to metal point contact. And then there were also experiments done having two rectifiers back to back — you could also do this with the wedges; if there are surface states when you apply a voltage, you can build up charge at the interface so that the voltage can divide between the part in which the direction is easy flow, and the part where it's high resistance. And if you have a charge at the interface, this means that there must be some state to hold that charge, which, another experiment confirmed the idea of surface state. So there were quite a few experiments done, a large number on contact potential measurements, a large number on the rectification characteristics which helped confirm the idea of surface states.

**Hoddeson:**

Most of the experiments were done by Brattain and Pearson?

**Bardeen:**

By what?

**Hoddeson:**

Excuse me? Brattain and Pearson?

**Bardeen:**

Well, Brattain was doing these experiments on surfaces.

**Hoddeson:**

On surfaces — and Pearson?

**Bardeen:**

And Pearson was as I said doing experiments on measuring the bulk properties of silicon. He prepared a series of specimens doped at different levels. One of the things one had to confirm is, doping with group 3 and group 5 compounds, do these really do go in interstitially as a simple picture

indicated? The way he did this is to make very accurate X-ray measurements, to show that the impurities really do go in substitutionally not interstitially. They substitute for the, group 3 and group 5 elements substitute for the silicon or the germanium. We later published this in a joint paper I wrote with Gerald Pearson on the electrical properties of silicon, which covered that topic pretty thoroughly, and the temperature dependence mobility, reasons for it, all the - a wide variety of things on the effect of doping. After the transistor came out, and people got interested in semiconductors, this paper got very wide distribution.

### **Hoddeson:**

What about Shockley? Was he doing experiments on that in this period also, aiming to understand surface states, or calculations, or was he working on something different?

### **Bardeen:**

He was not actively engaged in it himself. He was interested, and suggested some experiments, which Brattain carried out. He didn't — he did some of the

surface, the field effect experiments himself, or in collaboration with others.

**Hoddeson:**

Did he continue trying to do field effect experiments in this period after the surface state hypothesis had become accepted?

**Bardeen:**

The only one I remember, I think it was with silicon - when he lowered the temperature so that the electrons were frozen in the surface states and observed an effect, but much smaller than expected. And attributed that to the low mobility in the films.

**Hoddeson:**

Once again, in Brattain's account, he describes these experiments also, and then discusses a single experiment that I also discuss in my paper, where he noticed that the water affected the results. That led to the work with electrolytes.

**Bardeen:**

Yea, it became apparent that the water affected the surface properties. Later, there was his experiment

showing how you modified the surface barrier by humidity. This was after the transistor had come out, but we already knew the format, that modifying the surface properties by various sorts of surface treatments — and humidity and moisture and so on, were things which affected the surface.

### **Hoddeson:**

Anyway, when one reads Brattain's account or his interview transcript, it looks as though that experiment in which he noticed the effect of the water suddenly, after he discussed the experiment with Gibney and Gibney made the suggestion of modulating it with applied field, which then gave you the idea, that geometry in which one could actually observe field effect - that it was almost an accident that the group returned to active pursuit of the field effect amplifier.

### **Bardeen:**

During that period, Shockley was heavily involved in other experiments, and the theory of dislocations, and also, flow of electrons through insulators like alpha [??] haloids and silver haloids, in which he learned about, got interested in these as a result of



the trip we made, in the summer of 1947 — Bill Shockley and I, to Europe. He got much interested in dislocations after visiting Mott at Bristol. Mott and co-workers at Bristol. And so he wasn't, at the time we were doing these experiments, he wasn't so actively concerned himself. We discussed the experiments with him, but he wasn't actively working in them during that period.

**Hoddeson:**

One would think he would get quite excited about the fact that now, perhaps, field effect —

**Bardeen:**

— after we got things rolling, of course he did get very excited about it, and put his full time on it.

**Hoddeson:**

Yes. But this resumption of the field effect research looks, as I say, it looks almost as though it was an accident. But on the other hand —

**Bardeen:**

Oh, I wouldn't say it was an accident. It was part of an overall program.

**Hoddeson:**

Research...

**Bardeen:**

Perhaps partly accidental, but when you're doing experiments and have an overall picture in mind of what you're trying to do, experiments which might not be relevant ordinarily do become relevant, and are put into the larger picture.

**Hoddeson:**

Right. OK, my final question on the list concerns the accident at which the point contact was created. You say you had an accidental washing away of the oxide layer.

**Bardeen:**

I'm not sure whether it was that, or the fact that the oxide wasn't insulating, because it's difficult to grow an oxide to be — we know now that it would be difficult to do that experiment, even under the best conditions, grow a thin oxide on germanium, and then put a layer of metal under it. It can be done, with silicon; to make the MOS transistors, you grow

a thin layer of oxide on a silicon crystal, without great metal over it, as the basis for the MOS transistor. It would take about 15 years of advances in technology to be able to learn how to do that without introducing too many surface states at the interface, and make it really, make a really effective device out of it. One of the key things was to remove - we had to learn was how to remove all traces of metal impurities from the oxide.

**Hoddeson:**

So, the I don't know where I got this idea, that in washing off the glycol borate, Brattain had washed away the water soluble oxide layer. Is that true?

**Bardeen:**

Yes, that's what he says.

**Hoddeson:**

Yes. But you think that that's not what happened? That in fact, the oxide was there but it was not insulating? I don't know that —

**Bardeen:**

Well, I'm not sure. In any case, the oxide wasn't insulating. We know now that you can't grow a thin oxide on germanium with operative metal, and expect to have insulating contacts on it. So I think there's more to it than that.

**Hoddeson:**

I have to rewrite that section.

**Bardeen:**

But I think, growing the oxide and the treatment with the glycol borate helped produce the large inversion layer. At the surface. That was a good surface treatment, for getting transistor effect with point contacts.

**Hoddeson:**

Yes. I think you said last session that, I think Gordon asked you whether, if the — well, if the oxide had been insulating, whether —

**Bardeen:**

— we'd have probably had a field effect transistor.

**Hoddeson:**

Do you think that would have worked?

**Bardeen:**

Sure. That's what, I don't know if over half, but I wouldn't be surprised if over half the present transistors work on that principle. MOS transistors.

**Hoddeson:**

Yes. Well, was your set-up appropriate for observing that effect? If the oxide had been —

**Bardeen:**

Well, we should have observed the field effect. As I've suggested, the MOS transistor — you modulate the inversion layer of the electric field [???] Now, you can do it, get a surface so you can swing it all the way from the inversion layer to the accumulation layer. That is, you can — there are so few surface states that you can swing the band all the way over from one way to the other. From highly type. At the

surface. By application of the electric field. But if we ... (off tape)

### **Bardeen:**

thought that the field required over common [???] surface states, and modulate the inversion layer — it depends on the density of the surface states, as long as the density is smaller than the field required. And the experiment with the electrolyte we interpreted as finding very large electric field, because the ions are very close to the surface, and so you find a large electric field right at the surface, and it overcomes the surface states. And we wanted — in order to do a similar thing, we needed to, to have a large field with a moderate voltage, you have to have a very thin insulating layer. And that's what we tried to do, by going with a thin oxide layer on the germanium.

### **Hoddeson:**

But you said that, in that period, you didn't know enough about how to grow this oxide layer, to grow —

**Bardeen:**

— to make an insulating layer. We didn't know all the — at the start, whether we would be able to or not. We thought we'd try. Then we found it wasn't insulating. That may have been in part because of a, washing the glycol borate off, but the main thing is that in any case, it probably wouldn't have gotten us anywhere.

**Hoddeson:**

You wouldn't, if you hadn't washed the glycol borate off, you —?

**Bardeen:**

No, because I think even today, no one knows how to make an MOS transistor with germanium.

**Hoddeson:**

So the experiment couldn't have worked as a field effect transistor, the one that you —?

**Bardeen:**

On the basis of what we know now, no, it couldn't. Even so, it took 15 years, even though people knew

the principle of it, to be able to make it in practice. To get the materials so perfect that they had a surface free from surface states. The problem is that only very small densities of surface state is enough to spoil the effect. So you have to have very high deal material to conserve the effect. It took 15 years for the technology to get around to learning how to do that.

### **Hoddeson:**

You once said that if the accident, so-called accident hadn't occurred, the shorting out, the contact, that then you would have had an MOS and perhaps the field would have advanced much more rapidly. But in fact, it probably had to go the way it did, because the technology wasn't up to creating the proper oxide — at that time.

### **Bardeen:**

No, that would have been hard, but we could have made junction transistors by doping the surface, the way bipolar transistors are now. The first junction transistors are grown junction transistors, in which the doping was done by putting impurities in the melt as the crystal was grown. The early junction



transistors were wholly poorly only operated at relatively low frequencies. Even as late as 1952, point contact transistors had, they/we put in a lot of effort to try to improve the point contact transistors, and they had for many applications superior properties to junction transistors. One of the most important of these is the high frequency range. The point contact would operate at something like 15 megacycles, the junction only to 100 megacycles or so, 100,000 cycles, 100 kilocycles. So that junction transistors had to be improved and further developed. They were first applied to hearing aids, where you didn't have to have a very high frequency, and the next major step was to make them suitable for radio frequencies. This was done with silicon transistors Texas Instruments, and I think some of the Japanese people who worked with Sony had a lot to do with developing germanium transistors which operated in the radio frequency range. Later silicon transistors to operate in the television range. The junction transistors that are made in this country are very costly, which meant that they could only be used for very special purposes, where you need the performance you can get, and most of these are military and space

applications. So that the semiconductor manufacturers in this country are looking more to military and space applications, and left the field open to the Japanese to develop products for the consumer market.

**Hoddeson:**

Well, I think I can go back to work on this paper now. Thank you very much, for answering some questions that have been bothering me for a while.

